METHODS IN THE MEDITERRANEAN

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SUPPLEMENTUM CENTESIMUM TRICESIMUM QUINTUM

DAVID B. SMALL (ED.)

METHODS IN THE MEDITERRANEAN

TUTA SUB AEGIDE PALLAS · 1683 ·

METHODS IN THE MEDITERRANEAN

HISTORICAL AND ARCHAEOLOGICAL VIEWS ON TEXTS AND ARCHAEOLOGY

EDITED BY

DAVID B. SMALL

E.J. BRILL
LEIDEN · NEW YORK · KÖLN
1995

The paper in this book meets the guidelines for permanence and durability of the Committee on Production Guidelines for Book Longevity of the Council on Library Resources.

Library of Congress Cataloging-in-Publication Data

Methods in the Mediterranean : historical and archaeological views on texts and archaeology / edited by David B. Small.
p. cm. — (Mnemosyne, bibliotheca classica Batava. Supplementum, ISSN 0169-8958 ; 135)
Includes bibliographical references and index.
ISBN 9004095810
1. Mediterranean Region—Historiography. 2. Classical antiquities. 3. Archaeology and history—Mediterranean Region.
I. Small, David B. II. Series.
DE8.M47 1994
938'.0072—dc20 94-9844
CIP

Die Deutsche Bibliothek – CIP-Einheitsaufnahme

[Mnemosyne supplementum]
Mnemosyne : bibliotheca classica Batava. Supplementum. –
Leiden ; New York ; Köln : Brill.
Früher Schriftenreihe
135. Methods in the mediterranean. – 1994

ISSN 0169-8958
ISBN 90 04 09581 0

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PRINTED IN THE NETHERLANDS

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INTRODUCTION

Considerations of the integration of history and archaeology have been too often avoided in theoretical circles. Until very recently most scholars have followed Binford and Clarke's lead, which viewed archaeology and history as two very separate endeavors, and have shown little methodological or theoretical interest in the archaeology of cultures with documentary records.[1] But the field is beginning to change. Vigorous work is being undertaken in American historical archaeology, with the recent publication of volumes such as Little's *Text-aided Archaeology*, Beaudry's *Documentary Archaeology in the New World*, and the theoretical and methodological essays of early pioneers such as Stanley South, and more recently, Leone, Potter, and Crosby.[2]

The lead that American Historical archaeologists have taken remains mainly unchallenged. Although other regions may present equally rich opportunities for the conjunction of archaeology and history, few have chosen to reflect upon the means for such integration. The Mediterranean stands as a dramatic example. Despite the fact that its scholars have one of the most extensive archaeological and documentary data bases, and have been engaged in some kind of textual study and archaeological investigation longer than any other area in the world, extremely few have addressed the important issue of the relationship between archaeology and texts. Interest in this important question has been limited, and far too small in proportion to the full range of activities of Mediterranean archaeologists and ancient historians.

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[1] L.R. Binford (1983) *In Pursuit of the Past: Decoding the Archaeological Record* (London: 1983) 20–21; D.L. Clarke (1978) *Analytical Archaeology* 2nd ed. (London: 1978) 11–12.

[2] S. South (1977) *Method and Theory in Historical Archaeology* (New York: 1977); M.P. Leone and P.B. Potter, Jr. (1988) 'Issues in Historical Archaeology', in *The Recovery of Meaning*, eds. M.P. Leone and P.B. Potter Jr. (Washington: 1988) 1–22; M.P. Leone and C.A. Crosby (1987) 'Epilogue: Middle-Range Theory in Historical Archaeology', in *Consumer Choice in Historical Archaeology*, ed. S. Spenser-Wood (New York: 1987) 397–410. See also Potter's independent work: P.B. Potter, Jr. (1992) 'Middle-Range Theory, Ceramics, and Capitalism in 19th Century Rockbridge Country, Virginia', in *Text-Aided Archaeology*, ed. B. Little (Ann Arbor: 1992) 9–24.

But we should not paint too dim a picture. A small number of works have begun to address this topic: Snodgrass on archaeology and history, the recent overarching works of Bintliff and Knapp on the application of Annales paradigms, or more specialized views, such as Rowland's analysis of survey and documents or Jones' thoughts on archaeology and texts in Roman provincial studies.[3] But this production remains very much in isolation. Characteristically, Knapp's volume on the Annales approach incorporates but one contribution from the ancient Mediterranean, his own. The other contributors hail from Mesoamerica, the steppes of Asia, or Medieval Italy. This collective lack of interest in this topic is highlighted by a recent session at the 92nd (1990) annual meeting of the Archaeological Institute of America entitled, 'Perspectives on the past: putting together archaeology, history, and philology'. This session was rightly earmarked as an APA/AIA joint session, but included only four papers![4]

Obviously more can be done. The rich documentary base of the Mediterranean, with its official histories, religious texts,

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[3] Snodgrass has been, by far, the most interested in overarching issues of texts and archaeology in Classical archaeology. For his essays see: A. Snodgrass (1983) 'Archaeology', in *Sources for Ancient History*, ed. M. Crawford (Cambridge: 1983) 137–84, A. Snodgrass (1984) 'The Ancient Greek World', in *European Social Evolution*, ed. J. Bintliff (Bradford: 1984) 227–34, A. Snodgrass (1987) *An Archaeology of Greece: the Present State and Future Scope of a Discipline* (Berkeley: 1987); R. Jones has contributed to the issue of archaeology and texts in Roman provincial archaeology, see: R. Jones (1984) 'Social Evolution and the Western Provinces of the Roman Empire', in *European Social Evolution: Archaeological Perspectives*, ed. J. Bintliff (Bradford: 1984) 245–66; for Bintliff's general observations, see: J. Bintliff ed. (1991) *The Annales School and Archaeology* (Leicester 1991). Snodgrass has a contribution in this volume as well: 'Structural History and Classical Archaeology' pp. 57–72; Jones offers comments on the Annales school and the archaeology of the Roman empire: 'Archaeology, the *longe durée* and the limits of the Roman Empire', pp. 93–107; for Knapp on the Annales approach see: A.B. Knapp, ed. (1992) *Archaeology, Annales and Ethnohistory* (Cambridge: 1992), A.B. Knapp (1992) *Society and Polity at Bronze Age Pella: An Annales Perspective* (Sheffield: 1992); Lloyd may be found in J. Lloyd (1986) 'Why Should Historians take Archaeology Seriously?', in *Archaeology at the Interface: Studies in Archaeology's Relationships with History, Geography, Biology, and Physical Science, British Archaeological Reports International Series* 300, eds. J. Bintliff and C. Gaffney (Oxford: 1986) 40–51; Rowland's work is specifically tied to texts and survey, see: R. Rowland, Jr. (1992) 'Documentary Archaeology in Sardinia', in *Text-Aided Archaeology*, ed. B. Little (Ann Arbor: 1992) 149–60.

[4] See 'AIA/APA Joint Session: Perspectives on the Past: Putting together Archaeology, History, and Philology', *American Journal of Archaeology* 95 (1991) 307–08.

works of literature, and various public and private inscriptions, plus the massive amount of varied archaeological material, correlated through decades of empirical research to specific events and documented social change, promises to offer much more to the issue of archaeology and texts than these isolated scholars represent.

This volume sets out to demonstrate this promise. Its essays, by both archaeologists and historians, focus on ways in which we may explore the issue of archaeology and texts in Mediterranean studies, taking careful notice of particular regional forms of archaeological and textual evidence and offering cross-cultural insights to the larger archaeological world.

An Overview

Yet to appreciate the importance of these essays, we must understand their position in the context of current thoughts on archaeology and texts among Mediterranean scholars. Two fields of research, Classical and Levantine studies, have brought some focus to bear upon this question of using both texts and archaeology in social reconstruction.[5] Let us take the Classical first. Past attempts to look at the issue of texts and archaeology have focused on the question of effective linkage. Three analytical frames have dominated.

The first is the assumption that a great deal of linkage between archaeological and textual material can be achieved by one-to-one matching. Most of us would recognize this familiar practice in the textual identification of excavated sites, textual explanation for noticed destruction levels, and documentary identification of isolated buildings. In a certain way there is little at issue in this methodology, and many important links have been forged between archaeological and textual data. But there is an underlying trap in this method as well, that, if not understood, can result in some rather specious conclusions. Snodgrass has cast doubts on this matching game in a par-

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[5] It was unfortunately not possible to include work from Pharonic Egypt in this volume. In general, archaeology in historic period Egypt has remained under philological control. See B.J. Kemp (1984) 'In the Shadow of Texts' Archaeology in Egypt', *Archaeological Review from Cambridge* 3 (1984) 19–28.

I chose the term Levantine here, because the issue of coordinating texts and archaeology in the eastern Mediterranean spans more than Biblical studies.

ticular Classical perspective.[6] An uncritical connection between text and archaeology can often result in what he refers to as the 'positivist fallacy'. Archaeologists and historians have for too long assumed that what they determine to be major events in the past, gleaned through archaeological research, necessarily connect with what appear to be major events in textual documentation, or vice versa. As he observes, this fallacy often produces incorrect histories of sites, with fallible connections between historical events and archaeological data.

The second assumed approach for linkage between texts and archaeology proposes that the textual record is a frame, in other words, the principal source against which archaeology plays a secondary role. This assumption is widespread, and not surprisingly, is held by both archaeologists and historians, being most recently expressed by the author of the most widely used text in Greek archaeology.

> The situation is somewhat different in regard to historic Greece. Here, apart from the obvious value of the recovery and study of monuments of Western art, archaeology makes its major contribution by filling in details and adding to history.[7]

One could easily reply here that Biers' remark does not originate from serious reflection on the tie between archaeology and history, but even among those who are at least known for fuller considerations of this problem, the assumption that the historical record is somehow a sturdier source than the archaeological is widely held. In discussing the issue of archaeology and history, Snodgrass recently echoed an earlier proposition of Stanley South and David Clarke, that historical documentation makes historical period archaeology robust enough to provide a testing ground for theories derived from prehistoric research.[8]

But, in reality, the textual record, either alone or in conjunction with archaeological data (historical archaeology) rarely finds itself so healthy. The assumption that textual data can be used either as a principal historical narrative, in which archaeology can add common-place information, or more theo-

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[6] Snodgrass (1983) (1987). Snodgrass is not alone in this observation, see G. Barnes (1984) 'Mimaki and the Matching Game', in *Archaeology and Texts, Archaeological Review from Cambridge* 3 (Cambridge: 1984) 37–47.

[7] W. Biers (1987) *The Archaeology of Greece: An Introduction* (Ithaca: 1987) 20; See also Biers (1992) *Art, Artefacts, and Chronology in Classical Archaeology* (London: 1992) 1–8.

[8] Snodgrass (1987) 227; South (1977).

retically, as a true historical record against which prehistoric models of behavior can be tested, is misleading. There are two issues here. First, as Finley clearly argued in a seminal review of the problem of the use of texts in historical research, texts are riddled with problems.[9] They are biased, most of what we possess is about the elite educated classes. They also display alarming gaps in information. Referring to the Classical world specifically, Finley isolated a serious lack of information for early periods in Roman history and for cities other than Athens. Second, texts are not a reflection of past society and therefore easily comparable to archaeological data, but must be seen as often parallel tangible products of past social strategies. Finley's review of Thucydides, the ancient Greek historian who is most often considered the most accurate, easily demonstrates this point. The ancient Greek and Roman concept of writing history was very much different from that today. Thucydides in his introduction freely explains that he has constructed speeches and their contexts in a manner which best fits his sense of importance. Thucydides is creating the past, his reality is confessed commentary.

Recent research however has taken the principle that the historical record can provide a primary analytical frame to a different level. Much of this work has come out of Cambridge University, where recent scholars have united history and archaeology into a unidirectional analytical approach. As Snodgrass comments:

> ... one of the most important changes now taking place in classical studies is the breaking-down of the barriers between these two disciplines (history and archaeology). I am constantly in contact with research students, both in Cambridge and beyond, who would be hard put to it to say in which of the two subjects they are working. This is happening precisely because both are moving sharply away from the traditional 'guild practices' of their predecessors. It may be freely admitted that the historians were the first to do this: ...

One cannot deny that Cambridge's combination of archaeology and history has provided scholarship with a new useful direction. But, even here, perhaps because ancient historians have been so forceful in directing new questions, the text retains its old privileged position, although now admittedly

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[9] M.I. Finley (1988) *Ancient History: Evidence and Models* (New York: 1988) 7–26.

[10] Snodgrass in Bintliff, ed. (1992) 62.

configured in a more modern analytical frame. Work produced by students of the "Cambridge School" demonstrates this point well. One of the first, Osborne's penetrating work with the social and economic structure of Classical Attica, starts with an observation that isolated farmsteads were not mentioned in the Classical texts and seeks to confirm that observation through archaeological data.[11] Morris, who is perhaps the graduate who enjoys the highest profile in terms of demonstrated use of archaeological and anthropological methods, likewise finds his theoretical constructs grounded in historical rather than archaeological material. His use of mortuary data to analyze social change in early state Greece, is important for its use of methods not normally employed in Classical studies, but its foundational premise, that there was a significant change that was correlated to a chattel slave owning society, is an historical observation taken directly from Finley.[12]

Textual data and its analysis is privileged in these cases, even though we may all agree with Snodgrass that this represents a significant improvement over previous approaches. But Classical archaeology in this sense is still an extension of history, and in being so is still limited to offering assistance to localized questions of interest to ancient historians, not capable of extension to any cross-cultural utility. The quality of this recent work is a definite benefit for ancient history, but its scope offers little to the archaeologist interested in broader questions of social configuration and change.

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[11] R. Osborne (1985) *Demos: The Discovery of Classical Attika* (Cambridge: 1985).

[12] I. Morris (1987) *Burial and Ancient Society: The Rise of the Greek City-State* (Cambridge: 1987). For Finley's models see M.I. Finley (1980) *Ancient Slavery and Modern Ideology* (London: 1980), M.I. Finley (1982) 'Problems of Slave Society: Some Reflections on the Debate', *Opus* 1 (1982) 201–11. Morris strongly argues that archaeologists adopt historians' models in historic period archaeology. 'I have also hoped to show how archaeologists concerned to understand culture change will find models derived from historians' works every bit as useful as those drawn from ethnology...' (1987: 213) see also Morris (1994) *Classical Greece: Ancient Histories and Modern Archaeologies* (Cambridge: 1994) 45–46. As I demonstrate in my own contribution to this volume, he further argues that the archaeological record can offer a context in which to observe the dynamic of models that have been constructed from historical analyses. The only graduate to actually separate herself from this textual overlay has been Alcock, who treats the study of Roman period Greece from a survey-dominant approach. See Susan Alcock (1993) *Graecia Capta: the Landscapes of Roman Greece* (Cambridge: 1993). It is perhaps worthy to note here that Alcock differs from the other Cambridge graduates in that she holds an undergraduate degree in anthropology.

The recent rise in the importance of rural survey represents the last important movement in the relationship between texts and archaeology in Classical studies.[13] Data coming from survey has been difficult to correlate with ancient textual data, and a sentiment shared by many historians and archaeologists is that the two are not very compatible. This has been especially acute in the study of the Roman provinces, where much of our recent knowledge comes from archaeology.[14]

But as I argue below in my observations on Hitchner, some archaeologists now find an adoption of paradigms from the French Annales School, which can structure the relationship between survey data and textual narrative on different scales, heuristically useful in combining these quite different forms of evidence in studies of the ancient Mediterranean. It remains to be seen how useful these paradigms will prove to be, however. Several authors have expressed skepticism, noting that the trend to incorporate Annalist paradigms seems a passing fashion, on line with simulation studies or structuralism.[15] Specifically, Bulliet reminds us that even after the adoption of some Annalist paradigms, the health of archaeology remains fixed to its ability to generate its own approaches.[16] In many ways these models do little more than reinforce the demonstrated utility of processual approaches in supplying environmental and economic patterns that help in giving meaning to short term events documented in the historical record.

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[13] Survey is one of the best features of Classical archaeology. Those interested might refer to Cherry's earlier overview, which is still valid in many of its observations: J.F. Cherry (1983) 'Frogs Round the Pond: Perspectives on Current Archaeological Survey Projects in the Mediterranean Region', in *Archaeological Survey in the Mediterranean Area,* eds. Dr. Keller and D.W. Rupp (Oxford: 1983) 375–416; More recent publications which provide insight in the practice of survey in the Mediterranean in general are G. Barker and J. Lloyd eds. (1991) *Roman Landscapes: Archaeological Survey in the Mediterranean Region* (London: 1991); J.F. Cherry, J.L. Davis, and E. Mantzourani (1991) *Landscape Archaeology as Long-Term History: Northern Keos in the Cycladic Islands from Earliest Settlement until Modern Times* (Los Angeles: 1991).

[14] See Jones' comments in R. Jones (1984) 'Social Evolution and the Western Provinces of the Roman Empire', in *European Social Evolution: Archaeological Perspectives,* ed. J. Bintliff (Bradford: 1984) 245.

[15] See comments outlined in C. Chippindale (1993) 'Ambition, deference, discrepancy, consumption: the intellectual background of a post-processual archaeology', in *Archaeological Theory: Who Sets the Agenda?* eds. N. Yoffee and A. Sherratt (Cambridge: 1993) 21–27.

[16] R.W. Bulliet (1992) '*Annales* and Archaeology', in *Archaeology, Annales, and Ethnohistory,* ed. A.B. Knapp (Cambridge: 1992) 131–34.

The other major field in which some scholars have questioned the correlation of texts and archaeology is Levantine studies. Recent discussions, particularly in Biblical studies, have had this focus.[17] At present there are two areas in which the combination of archaeology and texts is seen to hold promise: the exploration of the full religious and cultic practices of early Israel, and the incorporation of settlement studies into models of the rise of the Israelite state. In reference to the first, Dever and Ahlström point out that archaeology has supplied alternative views of the religion and cults of early Israel, by supplying evidence for other deities worshipped along with Yahweh.[18] Settlement studies, which were always of some archaeological interest, made their first serious impact upon the recreation of Israel's past with the publication of Finkelstein's work with early Iron Age settlement.[19]

In general the combination of archaeology and texts is considered beneficial: recovering Israel's past environmental setting—here in a broad sense—neighbors, climate, topography, agricultural systems, etc.; clarifying and illuminating enigmatic biblical texts; adding missing elements of the biblical story, from accounts of defeats not in the Bible to pagan cults; and bringing

[17] This has occurred over the last 15 years. For essays on this topic see: J. Maxwell Miller (1977) 'Archaeology and the Israelite Conquest of Canaan: Some Methodological Considerations', *Palestine Exploration Quarterly* 109 (1977) 87–93, J. Miller (1987) 'Old Testament History and Archaeology' *Biblical Archaeologist* 49 (1987) 51–62, J. Miller (1991) 'Is it Possible to Write a History of Israel without Relying on the Hebrew Bible?', in *The Fabric of History: Text, Artifact and Israel's Past*, ed. D.V. Edelman (Sheffield: 1991) 93–102; W.G. Dever (1972) *Archaeology and Biblical Studies: Retrospects and Prospects* (Evanston: 1972), W.G. Dever (1990a) 'Artifacts, Ecology, Texts, and the Bible', in *Recent Archaeological Discoveries and Biblical Research* (Seattle: 1990) 3–36, W.G. Dever (1991) 'Archaeology, Material Culture and the Early Monarchical Period in Israel', in *The Fabric of History: Text, Artifact and Israel's Past*, ed. D.V. Edelman (Sheffield: 1991) 103–15; G.W. Ahlström (1991) 'The Role of Archaeological and Literary Remains in Reconstructing Israel's History', in *The Fabric of History: Text, Artifact and Israel's Past*, ed. D.V. Edelman (Sheffield: 1991) 116–41; T. Thompson (1992) *The Early History of the Israelite People, from Written and Archaeological Sources* (Leiden: 1992).

[18] For fuller information see W.G. Dever (1991) and W.G. Dever (1984) 'Asherah, Consort of Yahweh? New Evidence from Kuntillet "Ajrud"', *Bulletin of the American Schools of Oriental Research* 255 (1984) 29–37; and A. Mazar (1982) '"The Bull Site": An Iron Age I Open Cult Place', *Bulletin of the American Schools of Oriental Research* 247 (1982) 27–42.

[19] I. Finkelstein (1988) *The Archaeology of Israelite Settlement* (Jerusalem 1988).

to light the realia of ancient ideology. These considerations have led Dever to propose that archaeology decouple itself from the biblical texts and become generally Syro-palestinian.[20]

Yet, these benefits seem rather limited, and keyed specifically to a systemic/environmentalist approach of processual archaeology. They seem unconcerned with developing effective methods of coupling.[21] Knapp's reference to an analytical impasse on the role of Egyptian/Levantine relations is a perfect example of problems generated by this limited scope.[22] Knapp himself however, represents an important exception to this trend in his interest in applying the Annales paradigm to the archaeology and history of the Levant and Pella in particular. But there have been few others.

Purpose of this Volume

There is obviously a need for fuller discussion of the role of archaeology and texts in Mediterranean studies. With few exceptions, works to date have dealt with general concepts or correlation, rather than focusing on specific issues and methods. The contributors to this volume however, have been asked to discuss the issue of archaeology and text, not in an overview, but with a special methodological focus, calling attention to particular features of these regional data bases, both archaeological and textual, whether they be *stelai*, the style of excavation reports, archival records, or the earliest histories. Since the issue of archaeology and texts is crucial to both ar-

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[20] W.G. Dever (1972) *Archaeology and Biblical Studies: Retrospects and Prospects* (Evanston Il: 1972), W.G. Dever (1990a) 'Artifacts, Ecology, Texts, and the Bible', in *Recent Archaeological Discoveries and Biblical Research* (Seattle: 1990) 3–36; for some flavor of the debate between text-oriented and artifact oriented scholars see: J.K. Hoffmeier (1989) 'Reconsidering Egypt's Part in the Termination of the Middle Bronze Age in Palestine', *Levant* 21 (1989) 181–93, W. Dever (1990b) '"Hyksos," Egyptian Destructions, and the End of the Palestinian Middle Bronze Age', *Levant* 22 (1990) 75–81, J.K. Hoffmeier (1990) 'Some Thoughts on William G. Dever's '"Hyksos' Egyptian Destructions, and the End of the Palestinian Middle Bronze Age"' *Levant* 22 (1990) 83–89.

[21] See, most recently, T. Thompson (1992). The issue of the conjunction of texts and archaeological material has not gone unnoticed in Palestinian archaeology. At the 1989 annual meetings of the Society of Biblical Literature/American Schools of Oriental Research there was a panel, 'The Role of History and Archaeology in Biblical Studies', which sought to address this question. The papers from the panel were published in D. Edelman, ed. (1992).

[22] A.B. Knapp (1992).

chaeologists and historians, I have asked both to contribute. In doing so, I have put historians into a new position in the dialogue over the connection between archaeology and texts. While major historians, such as Humphreys and Finley in Classics and Miller and Thompson in Levantine studies have written on the issue of archaeology and history, their position has been distant, analyzing the utility of general archaeological methods and interests, but not addressing themselves to the issue of archaeology and texts in a specific part of their own research.[23] Historians in this volume discuss applications rather than general concerns.

This volume can in no way stand as an exhaustive treatment of the issue of texts and archaeology in these regions. Rather, this volume aims at introducing innovative, pragmatic solutions to integration in the Mediterranean. Only a limited number of scholars are contributing here. But each contribution has been set to raise questions that apply to real examples, to begin the overdue process of working out what is practical, useful, and insightful in the search to find meaningful correlations between text and archaeology in ancient Mediterranean studies. The contributions fall under three general headings: setting an historian—archaeologist dialogue, addressing features of archaeology and text, which are specific to Classical and Levantine cultures, and setting work on text and archaeology within these cultures into a larger archaeological context.

What follows is an introduction to some of the principal arguments in each contributor's essay. Since the readership might not be aware of the analytical contexts for some of these approaches, I am supplying background where necessary.

Setting an Historian—Archaeologist Dialogue

Dyson

Two contributors, one an historian and one an archaeologist have produced essays that begin to build bridges between the archaeologist and historian. Dyson, the archaeologist, has sought

[23] For Classics see S. Humphreys (1978) *Anthropology and the Greeks* (London: 1978) 109–29; M. Finley (1988) *Ancient History: Evidence and Models* (New York: 1988). For Levantine see J.M. Miller (1991) 'Is it Possible to Write a History of Israel without Relying on the Hebrew Bible?'. in *The Fabric of History: Text, Artifact and Israel's Past*, ed. D.V. Edelman (Sheffield: 1992); Thompson (1991).

to see archaeology as text, amenable to social deconstruction. His analysis provides valuable insights, not only for the archaeologist, but for the historian who must understand archaeology.

Since the hegemony of new archaeology was successfully challenged in the early 1980s a whole host of new approaches to archaeology has surfaced. One of these approaches, the view of archaeology as text, both in its relationship to archaeological material and in its production of texts, has seen quite a bit of interest.[24] Most analyses of the textual nature of archaeology take a wide view, looking at the discipline of archaeology in general, or at best, localizing their focus to specific investigations, where concepts of power and society are presented.

What makes Dyson's contribution important within this context is that he applies this type of deconstruction to a specific type of archaeology—classical. In this approach he is most like Embree in his recent discussion, 'The structure of American theoretical archaeology: a preliminary report', where he focuses on a discussion of a specific type of archaeological approach.[25]

In viewing the archaeologist as a site reader and an author of archaeological texts, Dyson sets the foundation for future considerations of the nature of classical archaeology, and lays a bed upon which effective linkage of history and archaeology might some day evolve. As Wells has argued, one of the chief problems with the use of archaeological data by historians is that they do not understand the process by which archaeologists write archaeology.

> Historians who are not themselves also archaeologists tend often to accept the conclusions of an excavation report when evaluating the evidence; more often, perhaps, they take their archaeology from summaries prepared largely for the historian's benefit. From this a number of dangers arise. An archaeologist claims for instance to have found 'no evidence' for, shall we say, Augustan occupation at a particular site; the earliest pottery and coins were Flavian. The unwary historian repeats,

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[24] See M. Shanks and C. Tilley (1987) *Reconstructing Archaeology* (Cambridge: 1987) I. Hodder (1989a) 'Writing Archaeology: Site Reports in Context', *Antiquity* 63 (1989) 268–74, I. Hodder (1989b) 'This is Not an Article about Material Culture as Text', *Journal of Anthropological Archaeology* 8 (1989) 250–69; C. Tilley (1989) 'Excavation as Theatre', *Antiquity* 63 (1989) 275–80, C. Tilley, ed. (1990) *Reading Material Culture* (Oxford: 1990).

[25] L. Embree (1989) 'The Structure of American Theoretical Archaeology', in *Critical Traditions in Contemporary Archaeology: Essays in Philosophy, History and Socio-Politics of Archaeology*, eds. V. Pinsky and A. Wiley (Cambridge: 1989) 28–37.

'This was not an Augustan site, but was first occupied in Flavian times', and uses this to elucidate Flavian strategy. Fuller study of the excavation report might however show that the archaeologist only dug in narrow trenches and over a very limited area of the site, and stopped when he got to the earliest stone buildings, with which he found Flavian pottery associated.[26]

Hedrick

An historian, Hedrick deconstructs history to outline the structural correlation between objects and texts. His work is singular in its attempt to find the beginnings of archaeological analysis in the writings of Herodotus and Thucydides. As scholars who were using both texts and objects to recreate the past, it is fitting that both of these Greek historians should be seen as wrestling with universal problems of correlation at the very inception of history.

As an historian Hedrick provides an historian's perspective on many of the central issues of archaeological meaning. And it is perhaps here that dialogues between ancient historians and archaeologists can best be constructed. Let me address two points. First, Hedrick focuses on the concept of the independent meaning of the object and the early historian's (Thucydides) recognition of that sense. This recognition can provide a point of discussion between archaeologists and historians. Hedrick, as an historian would see independent meaning as an essential starting point for analysis, but this challenges the current theoretical positions of many archaeologists who would argue that meaning can only be context-constructed.[27] Historians and archaeologists need to come to terms with this cross disciplinary issue.

Second, Hedrick's perceptive analysis of Thucydides' use of power instead of material remains (his available archaeological record) as a scale of reality is also extremely important.

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[26] C. Wells (1984) *The Roman Empire* (Stanford: 1984) 47–48.

[27] For some specific examples, see the early work of Hodder: I. Hodder, ed. (1982) *Symbolic and Structural Archaeology* ed. I. Hodder (Cambridge: 1982), I. Hodder, ed. (1987) *The Archaeology f Contextual Meanings*, ed. I. Hodder (Cambridge: 1987). While Hodder has led an early foray into contextually-generated meaning in archaeology, several others have been quick to identify with this approach as well. A contextually-focused analysis is currently making significant inroads in Mesoamerican studies. See, for example. L. Schele and D. Freidel (1991) 'The Courts of Creation: Ballcourts, Ballgames, and Portals to the Maya Otherworld', in *The Mesoamerican Ballgame*, eds. V.I. Scarborough and D.R. Wilcox (Tucson: 1991) 289–316.

Archaeological meaning is extremely slippery and variable, and Hedrick's historical focus offers some hope that historians and archaeologists could fruitfully discuss the variable scales of meaning within artifacts and how they are used in the creation of history.

Specific Mediterranean Features

Ober

The Mediterranean provides a variety of artifacts and texts, which, although not exclusive to the region, are widely found there, and present inherent constraints and opportunities for effective correlation of archaeology and texts. Three contributors have focused on inscribed *stelai*, standing monument cemeteries, and land tenure documents.

Ober examines the historian's use of a specific type of inscribed *stele*, the *horos*. In examining the relationship between text (as artifact) and its context, Ober is following a recent tradition in prehistoric Mediterranean analysis. In 1984, Bennet sought a broad approach to situating Linear B texts in their archaeological contexts, and developing a subsequent analysis.[28] In the same vein, Palaima and Shelmerdine; and Palaima and Wright, sought to emphasize that the context of the Linear B tablets (here at Pylos) was just as important as their epigraphical information.[29] Specifically, Palaima and Shelmerdine argued that an analysis which combined the writing of the tablets with their architectural context, could produce a model of the industrial organization of the Pylian palace industry. Different types of records on the tablets were spatially distinct in the palace. This spatial organization should parallel a similar division of labor and authority in the crafts production. Twisting this analysis a bit, Wright and Palaima argued that the model of the organization of the palace crafts industry, gained from an analysis of the texts and their con-

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[28] D.J. Bennet (1984) 'Text and context: levels of approach to the integration of archaeological and textual data in the Late Bronze Aegean', *Archaeological Review from Cambridge* 3 (1984) 63–75.

[29] T.G. Palaima and C.W. Shelmerdine (1984) 'Mycenaean archaeology and the Pylos texts', *Archaeological Review from Cambridge* 3 (1984) 76–89; T.G. Palaima and J.C. Wright (1985) 'Ins and out of the archives rooms of Pylos: Form and function in the Mycenaean palace', *American Journal of Archaeology* 89 (1985) 251–63.

text, could be used to suggest reconstructions of parts of the destroyed palace. This movement to combine text and context in social analysis had been limited to Bronze Age research in the Mediterranean, until Ober.

Ober's approach, although it seeks the same goal as many archaeologists, is different in its methodology. It differs in that its analytical reference is to speech act theory, rather than archaeological discussions of an object-oriented contextual creation of meaning.[30] In his case this presumably stems from his earlier research in Athenian rhetoric. The result, that text is context-dependent and that context is not dynamically immutable, is however, the same as that which would stem from a more purely archaeological interpretation. The conjoining of these two approaches in the future could be extremely profitable in the effective linking of text and archaeology.

As a final note, Ober's concentration on the *horos*, or *stele*, points to possible productive cross-cultural research. Inscribed *stelai* are also found in Mesoamerica, where Mayan rulers set them up to advertise their reigns. Like Ober, Freidel and Schele have also sought to derive meaning for inscribed *stelai* from their wider archaeological context, recreating meaning from the integration of textual *stelai* and the architectural landscape of Maya urban centers.[31]

Hitchner

Hitchner's contribution aims at a specific type of archaeological and textual link in survey studies. The use of survey has increased greatly within the general Mediterranean over the last 25 years. I have already mentioned that the recent work of Finkelstein made a significant impact upon the value given

[30] See I. Hodder (1987) 'The Contextual Analysis of Symbolic Meanings', in Hodder, ed. (1987) 1–10, as well as Hodder, ed. (1982), Freidel and Schele (1991).

[31] D. Freidel and L. Schele (1990) *A Forest of Kings: the Untold Story of the Ancient Maya* (New York: 1990). For a model of meaning generated from the integration of textual stelai and the wider archaeological context, see chapter 8, 'Copan: the death of First Dawn on Macaw Mountain'. One should also note here that the issue of archaeology and texts in Mesoamerica is becoming quite contentious. A rift is developing between epigraphers and archaeologists. For the most recent archaeologists' attack on the interpretation of Mayan texts by epigraphers like Schele (Freidel is a practicing archaeologist as well as an epigrapher), see J. Marcus (1992) *Mesoamerican Writing Systems: Propaganda, Myth, and History in Four Ancient Civilizations* (Princeton: 1992).

to and use of survey in Levantine studies. For Classical, the role of survey has a longer history. Since the pioneering work of MacDonald in his Messenia Survey, and that led by Ward-Perkins shortly after the second world war in southern Etruria, the pace of archaeological survey in Classical studies has quickened and it is now seen as an important source of information on past Classical civilizations. Its importance has been increased by two factors: the rapid disappearance of many rural sites due to modern development and ancient historians' recent focus on ancient rural economics.

A classic problem with the integration of survey data and textual information is that, on the surface, the two do not seem to stem from the same culture. In general, textual information is keyed to events within cities, the political or military deeds of great people, while survey information pertains to the textually invisible, the farmer or landowner, whose economy kept the cities going.

While the differences in these types of data would seem to make an integration of the two impossible, many would now say that integration is possible, through the adoption of Annales paradigms. As a movement which started in the earlier decades of this century, the Annales School (if we can truly call it that), has approached history from a wide angle. Its practitioners argued that the recreation of the past should start from a multidisciplinary approach that combined textual analysis with such disciplines as geography, sociology, anthropology, and psychology. They additionally firmly recognized the importance of quantification. This movement has fostered a number of analytical paradigms, two of which have been currently applied to study in the Mediterranean. Perhaps the more visible is Braudel's theme of the interrelation of three scales of time: the longue durée, or time which responds to environment/culture relationships, and is capable of spanning a millenium or more; the conjoncture, or time that is seen in spands of prices, wages, accounts, demographics, technical change, et al, and stretches to 5 to 50 years; and l'histoire événementielle, or time that is seen in political narrative, the shortest time of all.

One can quickly see the attractiveness of this paradigm. Survey information would neatly fall on the longue durée scale, being hard economic data correlated to environmental frames that would correspond to the slow process of change usually seen

in the countryside. Textual material would correspond to the événementielle, and supply a political narrative, keyed to short term events, that might be epiphenomenally related to the dynamic of the longue durée.[32]

The second paradigm that has been applied has been taken from Le Roy Ladurie, who was trying to correct a deficiency in the text/longue durée correlation that he saw in Braudel's work. Roughly termed structure/event/structure, this paradigm seeks to relate documentary evidence to material by witnessing the creation of long term structure by narrative events. Knapp's example illustrates this goal.[33] In analyzing the relationship between Egyptian texts and the archaeological data of the Levant in the Late Bronze Age, he argues that the texts join with the archaeological data at a point where the structure of the archaeological data is changed by the behavior indicated in the texts. Egyptian hegemony in the Levant (the structure and seen in archaeological data) was changed by the Hyksos domination of northern Egypt (an event and seen in textual data). As a result of the Hyksos incursion the subsequent Egyptian domination of the Levant was changed (new structure and seen in the archaeological data).

Hitchner is also searching for a useful means of linking survey data to texts, but his approach, rather than borrowing a larger paradigm from French historical studies, seeks to isolate structure within specific types of texts, land tenure documents, and then correlate elements of that textual structure to the landscape. In his research Hitcher closely parallels Davis's recent work with Ottoman records and survey in the Aegean.[34] By analyzing Ottoman documents recording land tenure, agricultural prices, and tax records, Davis was able to reconstruct a structure of land holding and a dynamic for change, which he has applied to noticed change in an earlier period from the survey record from Kea. Unlike Davis however, Hitchner here is very specific. In his analysis he compares terms of land holdings to

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[32] I am abbreviating a synthesis of opinions here on the proper use of the Braudel's scheme. One divergent, and very interesting opinion comes from Snodgrass, who argues that there is room for focusing on the *conjoncture*, by incorporating villages into the correlation. See Snodgrass (1991) 'Structural History and Classical Archaeology', in Bintliff, ed. (1991) 57–72.

[33] Knapp, ed. (1992), Knapp (1992).

[34] J.L. Davis (1991) 'Contributions to a Mediterranean Rural Archaeology: Historical Case Studies from the Ottoman Cyclades', *Journal of Mediterranean Archaeology* 4 (1991) 131–216.

the survey record and returns with an archaeological identification of terms such as *fundi*, *loci*, and *agri*.

Small

My own contribution falls under this theme of specifics as well. In it I make two arguments. First, although more and more studies of social configuration and change in ancient Greece now correlate mortuary data with written material, neither archaeologists nor historians have understood the unique structural characteristics of the cemeteries they are using. Most of these cemeteries I classify as 'standing monument cemeteries' i.e. cemeteries with more or less permanent markers. Current studies overlook an important inherent contextual principle for standing monument cemeteries, which significantly alters currently held concepts of the strategic meaning of Athenian sumptuary legislations. That principle is simply that such legislation will create a strong distinction in the context of the cemetery between families and individuals that have already been able to use the cemetery for status claims and those who are now denied that opportunity.

My second argument addresses the issue of textual privileging in Classical studies. Using my work with cemeteries as an example, I argue that text privileging has contributed both to the prevalent myopic focus on Athens' archaeological record, and to incorrect reconstructions of large scale social configurations. The special character of Athens' cemeteries underscores the need for historians and archaeologists to try to view the archaeological record as independent, capable of supplying material and structure that is equal to the historical.

CROSS-CULTURAL VIEWS

A serious shortcoming in Mediterranean archaeology is that it rarely connects with archaeology from other regions in the world. Mediterranean research is thus too isolated, when it could profit from analysis that includes its social structures in larger analytical frames that also study cultures as diverse as Mesoamerican, historical North American, as well as Southeast Asian. Work with archaeology and texts in Mediterranean studies, if it is ever going to enter larger debates on past culture, must also be of cross-cultural value.

The last three papers in this volume treat cross-cultural is-

sues from a definite Mediterranean perspective. But their value lies not so much in illuminating local features, but in furthering the general development of the integration of archaeology and texts.

Kosso

Kosso's contribution is the most theoretical, addressing the issue of epistemic independence between archaeology and texts. The independence of archaeological and historical data has recently been seen as an avenue into past meaning rather than an excuse for throwing in the analytical towel. The benefits produced by working with this independence were recognized in the early 1980s by Carmack and Weeks, two Mesoamerican archaeologists, who were working with the post classic Quiche peoples.[35] They termed their approach 'conjunctive' and claimed that

> ... contradictions found between the material (archaeological) and symbolic (ethnohistoric) reconstructions may be as important as functional correlations. That is, our research may lead us to discover the 'dialectical' nature of past social life—the way the demands of material conditions provoke behavior which may be out of step with social norms and beliefs.[36]

They applied this focus on the dialectic to their study of the social structure of the people at Utatlan. Their documentary record indicated a strong hierarchy of lineages, but their material record, the spatial analysis of Utatlan, indicated a more equal moeital distribution of power. With this difference highlighted they returned to the texts to look for a structural parallel to that on the ground, and found a possible division of power between two lineages, the Cawek and the Nijaib. Further analysis of the spatial make up of the site supported this ethnohistoric parity, but on a somewhat different spatial division than the authors first expected.

Few people have followed Carmack and Weeks' lead, but a parallel approach was advocated shortly after by Leone, Potter and Crosby.[37] These archaeologists sought to apply Binford's concept of middle-range theory to historical archaeology. In

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[35] R. Carmack and J. Weeks (1981) 'The Archaeology and Ethnohistory of Utatlan: A Conjuctive Approach:, *American Antiquity* 46 (1981) 323–41.
[36] Carmack and Weeks (1981) 39.
[37] See Leone and Crosby (1987); Leone and Potter (1988); Potter (1992).

calling for analytical rigor in the use of ethnoarchaeology in archaeological explanation, Binford proposed that we carefully compare ethnoarchaeological records with archaeological.[38] He argued that ethnoarchaeological data should be analyzed to produce a grid from which expectations can be made for the archaeological record. Where these expectations are not met in the archaeological record, that is, where a differential gap in the two sets of data is detected, is called ambiguity. This ambiguity should then be analyzed in reference to suggested organizational behavior, gleaned from other sections of the ethnoarchaeological record. Ambiguity is then seen as the critical field of investigation, one that can supply fuller specific meaning to the archaeological record.

Leone, Crosby and Potter suggested that we operate in an analogous fashion in historical archaeology. They argue that we view both the archaeological and documentary record as independent. In this view, instead of working from an ethnoarchaeological script, we should substitute for it the documentary record. We can develop an analytical grid from the analysis of recorded folk taxonomies or noticed sets of rules in the textual material, which will generate expectations for the archaeological record. Other parts of the documentary record can be analyzed as well to arrive at concepts of organizational behavior that can supply meaning to any noticed ambiguity rising from the application of expectations coming from an analysis of documentary material.

Leone's work here is important and bears an example.[39] In his study of the 18th century William Paca Garden in Annapolis, Maryland, Leone developed a grid from analysis of 18th and 20th century gardening books. From his reading of 18th century books on gardening, which sought to describe the garden as closer to a managed wilderness than a geometrically ordered space, he expected to see a structure within the Paca

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[38] L. Binford (1977) 'General Introduction', in *For Theory Building in Archaeology*, ed. L. Binford (New York: 1977) 1–10, L. Binford (1982) 'Objectivity—Explanation—Archaeology 1981', in *Theory and Explanation in Archaeology*, eds. C. Renfrew, M. Rowlands, B. Seagreaves (New York: 1982) 125–38. For a recent discussion of the position of middle-range theory in the processual/post processual climate, see P. Kosso (1991) 'Middle-Range Theory as Hermeneutics', *American Antiquity* 58 (1991) 621–27.

[39] M. Leone (1984) 'Interpreting Ideology in Historical Archaeology: Using the Rules of Perspective in the William Paca Garden in Annapolis Maryland', in *Ideology, Power and Prehistory*, eds. D. Miller and C. Tilley (Cambridge: 1984) 25–35.

garden that matched the wilderness rules. However, the Paca garden did not meet this expectation in that 2/3 of the space, that closest to the house, was geometrically arranged and resembled more the Renaissance gardens of Italy than those of the English homestead. This orderliness was the ambiguity, the disjunction between text and archaeology that signaled unique meaning for the William Paca garden. To give some sort of organizational meaning to this ambiguity, Leone analyzed other parts of the relevant documentary record, and was able to arrive at a concept of underlying organizational behavior for 18th century gardens. Gardens in this period were used for entertaining, and establishing and maintaining social position. Why would an ordered garden be a symbol in social negotiation, rather than one that is based on representing wilderness? His conclusion was that the rationality of the garden and the implied mastery of that rationality by the Paca family, was analogous to the prevalence of the mastery of society through the application of rational law, which was a strong theme within the context of colonial America.

Kosso's extension of this type of analysis is extremely important. He introduces middle range theory to Mediterranean studies, where the documentary record is abundant, giving Classical scholars in particular an avenue into new productive research. Cross-culturally, through his analysis of Greek archaeology and Greek historical texts he is able to modify Leone's original claim. While epistemic independence is often sought, it can ignore the distinction between texts and archaeology.

Hesse

As Hesse recounts, the Levant has produced a sizeable sample of faunal remains, second in abundance only to ceramic, and provides a rich arena for zooarchaeological research. Hesse is concerned here with the livelihood of zooarchaeology in the current postprocessual climate. Zooarchaeology has been defined more by its material data base than by any developing analytical position, and Hesse sets out to offer some positions that might prove useful to this specialized branch of archaeology. As he observes, zooarchaeology's relationship to texts is an important consideration in this exercise. Within documentary archaeology, zooarchaeology is still regarded as an adjunct, a nice addition to excavation reports, that is too often limited to an appendix. The survival of zooarchaeology is keyed

to its ability to move into a position that would allow it to pose questions that would fundamentally affect the direction of the entire archaeological project. To do so, it must effectively link with textual data. Hesse argues that we should compare patterns in the texts to patterns in the behavioral record, indicated by faunal analysis. In this comparison, most analytical power comes from a comparison of the texts to information gleaned from the traditional processual approaches in zooarchaeology. His discussion of the questions raised by a processual approach's interaction with specific pig prohibitions in ancient Israel parallels closely the paradigm taken from the Annales School, where the longue durée (here the record of pig husbandry and the environment) is seen as supplying fundamental underpinnings to urban (here temple) discourse.

On the reverse side of this issue Hesse also argues that zooarchaeology must adopt some of the analytical framework of historical archaeology, looking for possible connections between a husbandry dynamic (early Neolithic domestication) and considerations of social status, power, gender, et al. Flexibility here seems to be the key.

Wapnish

Leone urged that the comparison of texts and archaeology should subject the texts to folk taxonomic analysis before comparison. In archaeology this has been limited mainly to linguistic analysis of American historical probate inventories, which has sought to identify classificatory systems and interpret their dynamic change.[40] In her study of 17th and 18th century inventories from Potomac Virginia, Beaudy sought to identify changes in world view between the two centuries.

Wapnish's work, while it continues to explore uses of folk classification, is distinctly different in its use of classificatory models, and its application to literature. Philologically trained, but with an expertise in archaeology the match of any of its practitioners, Wapnish is well-suited to develop methods of linking texts and archaeological data. As she demonstrates, folk taxonomy can be a useful intermediary between philology, zoology, and archaeology. She is looking for conceptual cat-

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[40] M. Beaudry (1988) 'Words for Things: Linguistic Analysis of Probate Inventories', in *Documentary Archaeology in the New World*, ed. M. Beaudry (Cambridge: 1988) 43–50.

egories that would aid both philologists and archaeologists in understanding the cultural meanings of both philological and archaeological taxa.

What I find most immediately beneficial from her study bears directly on the analysis of archaeological data. As she argues in reference to animal bones specifically, this folk taxonomic analysis of texts uncovers culturally significant lines of investigation. In her case of the presence of dolphin bones, it is not the provenance of the bone that is as culturally filled with meaning as an understanding of the meaning of its folk specific category. Wapnish has made a significant start on work that can and should be cross-culturally expanded. One would like to see similar work with ancient and Medieval epics, such as the *Iliad* or *Beowulf,* or even the origin myths, such as the *Popol Vuh* of the Maya, to elicit some folk meaning to correlated archaeological data. There is a wealth of culturally-specific investigation waiting us.

SETTING AN HISTORIAN—ARCHAEOLOGIST DIALOGUE

IS THERE A TEXT IN THIS SITE?

STEPHEN L. DYSON

Contemporary literary and cultural theory is much concerned with the nature and use of texts. Old fashioned textual criticism, which was principally concerned with the establishment of the correct verbal form of important literary texts, and the New Criticism, which focused on the establishment of meaning working from within the structure of texts with minimum reference to either author, historical circumstances of production, or the interpretive traditions of generations of readers, have given way to radically different approaches.[1] These focus on considerations of how the contemporary ambience (or ambiences) shaped in different ways the author and what the author produced (New Historicism), or ways in which texts by means of their own internal contradictions betray their own authors and the author's intentions (Deconstruction). They additionally treat the manner in which different sets of historically, socially and culturally defined readers create a range of meanings from the same collection of texts (Reader Response Criticism).[2] The complexity and confusion inherent in this debate have recently been abundantly chronicled in the scholarly and literate popular press.[3] It is reflected in the title

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[1] R. Wellek and A. Warren (1955) *Theory of Literature* (New York 1955) lays out the historical background and basic tenets of the New Critical school. The approach of the New Critics is nicely captured in C. Brooks (1947) *The Well Wrought Urn* (New York: 1947). J. Culler (1988) *Framing the Sign* (Norman, Oklahoma: 1988) 9–15 places the New Criticism in the development of contemporary literary theory.

[2] The New Historicism is associated with such works of Stephen Greenblatt as (1990) *Learning to Curse* (New York: 1990). For an overview of the approach and its development, cf. Greenblatt (1990) 1–15 and H. Aram Vesser (1989) *The New Historicism* (New York: 1989). On Deconstruction, cf. J. Culler (1982) *On Destruction: Theory and Criticism after Structuralism* (Ithaca: 1982). For Reader Response Criticism see the references in n. 8.

[3] A good example of this is the controversy surrounding the World War II political activities of Deconstructionist critic Paul De Man and the relation between the supposed Nazi associations of De Man and the implicit ethical limitations of the Deconstructive method. These issues and concerns are the focus of D. Lehman (1991) *Signs of the Times* (New York:

of the very influential book by Stanley Fish *Is There a Text in This Class*, which provides the reference point for the title of this essay.[4]

Central to all of these newer movements in literary criticism is a concern with the complex interaction of author, text, and reader. It is fair to say that the current world of literary criticism considers the assertion of the death of the author as premature, even though such critics are unwilling to restore the writer to her/his former status of Romantic self-sufficient, creative figure. In contrast, the concept of the text as an entity to be studied independent of author and environment, has been increasingly abandoned in the post New Critical world. The importance of authorial environment, activity and intention has been reasserted by the New Historicists. They have taken the offensive against both the older New Critics with their notion of textual self sufficiency and the Deconstructionists with their emphasis on ways in which texts betray their authors, thus making the conveyance of authorial meaning an extremely difficult task.[5]

Readers have also returned with authors. Strict New Critics raised the bugbear of the 'affective fallacy', attacking the tendency of readers to transpose their own values and experience into texts.[6] In contrast, new generations of Reader Response Critics have argued for the importance of readers both individual and collective and for the tradition of readership in giving meaning to texts. Their approaches have ranged from the psychology of the reader to the identification of communities of readers who have helped shape collective interpretations.[7]

The notion of text has also been extended well beyond the elite products of the written word. Central here are the works of Michael Foucault with their assertions about the historical

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1991). Significantly professorial literary critics are about the only academics to achieve the trendy apotheosis of profiles in the *New York Times Magazine*. The spirit of these debates have been caught in novels of David Lodge such as (1984) *Small Worlds* (London 1984) and (1988) *Nice Work* (London 1988).

[4] S. Fish (1980a) *Is There a Text in This class? The Authority of Interpretive Communities* (Cambridge MA: 1980).

[5] S. Greenblatt (1990) note 3; Vesser (1989) note 3.

[6] W.K. Wimsatt and M. Beardsley (1954) 'The Affective Fallacy' in *The Verbal Icon: Studies in the Meaning of Poetry* (Lexington: 1954) 21–39.

[7] The varieties of approaches in Reader Response Criticism are presented nicely in the essays in J.P. Tompkins, ed. (1980) *Reader-response Criticism: from Formalism to Post-structuralism* (Baltimore: 1980).

and ideological construction of such basic concepts as sex and madness.[8] Feminist critics can talk about the body as a type of text, which will be 'read' differently in different cultural circumstances.[9]

Classical archaeologists have traditionally granted to the philologists and ancient historians the central position in the realm of text study. It is true that archaeologists use literary texts as they attempt to give meaning to material culture. One of the advantages that classical archaeology has over many other subfields in the discipline is the abundance of written evidence that can be used to substantiate deductions drawn from the study of material culture. Indeed, it can be argued that the research agenda and interpretive universe of classical archaeology have been excessively shaped by deference to the written text oriented world of classical philologists and historians.[10]

The classical archaeologist has tended to treat as separate conceptual categories the world of texts and the world of sites and artifacts. Philologists and historians are seen as having texts and the archaeologist artifacts and monuments. However, in the contemporary, more fluid definition of the word, archaeology is much concerned with its own special brand of texts derived from the world of material culture and with the intersection of traditional notions of the text with that of those material culture texts.

Archaeologists are also involved in the creation of a range of written texts produced for audiences of various levels of reading competence. In producing these varied texts the archaeologist combines the roles of text reader and text producer. The primary task of the archaeologist is that of establishing meaning in material culture, in itself a form of reading.[11] In establishing this meaning, the archaeologist works not only as

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[8] M. Foucault (1961) *Madness and Civilization* (London: 1961); M. Foucault (1976) *The History of Sexuality* (Harmondsworth: 1976). P. Caplan (1987) *The Cultural Construction of Sexuality* (London: 1987).

[9] M. Jacobs, E. Keller, S. Shuttleworth (1990) *Body Politics* (New York: 1990); E. Scarry, ed. (1988) *Literature and the Body* (Baltimore: 1988).

[10] S.L. Dyson (1989) 'Classical Archaeology' in *Classics, a Discipline and Profession in Crisis*, eds. P. Culham and L. Edmunds (New York: 1989) 211–13.

[11] I. Hodder (1991) *Reading the Past: Current Approaches to Interpretation in Archaeology*, 2nd Edition (Cambridge: 1991); I. Hodder (1988) 'Material Culture "Texts" and Social Change: A Theoretical discussion and some Archaeological Examples' *Proceedings of the Prehistoric Society* 54 (1988) 67–75; I. Hodder (1989) 'This is not an Article about Material Culture as Text', *Journal of Anthropological Archaeology* 8 (1989) 250–69.

an individual scholar, but also as part of a community of readers, whose collective values strongly shape what the individual archaeologist will discover and consider important in the material culture record.[12]

The archaeologist as reader rapidly converts to the archaeologist as author. Archaeological authoriality is extremely complex, in part because it operates in so many forms and aims at so many audiences. Archaeological publications range from preliminary reports and specialized monographs to synthetic overviews and popular potboilers. All of these texts are created by processes that involve complex questions of genre, canon and power, and relate author and reader in complex, diverse ways.

It is one of the several virtues of the post-processual archaeologists that they have called attention to ways in which contemporary literary and cultural historical concerns can be related to the world of archaeology.[13] A number of perceptive essays have attempted to explain to archaeologists the potential usefulness of Foucault, Derrida, and other 'in' authors in the fields of literary and cultural criticism.[14] The Postprocessualists have stressed that these currently fashionable modes of criticism can be used not only to interpret the past, but also to identify ways in which such factors as gender, class, and community tradition shape the manner in which archaeologists go about extracting meaning from the past. If their efforts can be faulted, it is in their failure to apply the new theoretical approaches to more specific archaeological situations.[15]

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[12] The concept of interpretative communities has been especially developed by Stanley Fish (1980a), see also S. Fish (1980b) 'Interpreting the *Variorum*' in *Reader-Response from Formalism to Post-Structuralism Criticism*, ed. J.P. Tompkins (Baltimore: 1980) 164–83. The importance of community standards in reading archaeological texts shapes the way in which certain students of the history of archaeology have read the development of the discipline. A classic work like G. Willey and J. Sabloff (1980) *A History of American Archaeology*, 2nd Edition (San Francisco: 1980) is built around Thomas Kuhn's model of radical paradigm shifts shaping the way scientists do their own research. Paradigms have to be encoded in individual human beings and that in turn creates the community of readers.

[13] In spite of the criticisms and reservations expressed in S.L. Dyson (1993) 'From New to New Age Archaeology: Archaeological Theory and Classical Archaeology—a 1990s Perspective', *American Journal of Archaeology* 97 (1993) 195–206, I find works like M. Shanks and C. Tilley (1987) *Re-Constructing Archaeology: Theory and Practice* (Cambridge: 1987) enormously stimulating.

[14] The collections of essays that I have found most useful are I. Baptry and T. Yates, eds. (1990) *Archaeology after Structuralism* (London: 1990); C. Tilley, ed. (1990) *Reading Material Culture* (Oxford: 1990); and R. Preucel (1991) *Processual and Postprocessual Archaeologies* (Carbondale: 1991).

[15] Archaeological sociology is dealt with in T. Patterson (1986) 'The Last

Archaeologists long considered themselves as inductive scientists, carefully and systematically accumulating information until answers became largely self evident. Research was done with increasing methodological sophistication, but with relatively little theoretical self reflection. The New Archaeologists questioned the old premises and modified the research model. They stressed the importance of a deductive approach in which the archaeologist formed primary hypotheses which were then tested by carefully crafted projects of field research. The image was still basically that of the rational scientist, but one that the New Archaeologists regarded as a more contemporary vision of science.[16] This paradigm has been increasingly challenged by both historians of science and post-modern cultural critics. Increasingly questions are being raised about the degree to which scholars working in the humanities, many fields of the social sciences, and even the natural sciences actually do research in the rational manner described by the New Archaeologists. Greater emphasis is being placed on the way in which scholars like historians create 'probable fictions' in a process closer to that of the creative writer rather than that of the scientist.[17] Related to these radical new interpretations are questions about the degree to which these scholarly creations are not just documents used in a truly objective scientific discourse, but also instruments for the maintenance of traditional power relationships both in academics and society.[18]

Sixty Years: Towards a Social History of Americanist Archaeology in the United States', *American Anthropologist* 88 (1986) 7–26. Questions of gender and archaeological interpretation have been perceptively considered in A. Wylie (1992) 'The Interplay of Evidential Constraints and Political Interests: Recent Archaeological Research on Gender', *American Antiquity* 57 (1992) 15–35, and J.M. Gero & M.W. Conkey eds. (1991) *Engendering Archaeology* (Oxford: 1991).

[16] On the inductive period in the development of archaeology, see also Willey and Sabloff (1990) 130–80, and on the New Archaeology and its impact, Willey and Sabloff (1990) 181–210. L. Binford (1962) "Archaeology as Anthropology", *American Antiquity* 28 (1962) 217–25; L. Binford (1972) *An Archaeological Perspective* (New York: 1972) esp. 1–14, captures the spirit of the New Archaeology at its most vigorous moment.

[17] Key works in shaping these new uncertainties about historical methods are H. White (1973) *Metahistory* (Baltimore: 1973) and P. Novick (1988) *That Noble Dream: The Objectivity Question and the American Historical Profession* (Cambridge: 1988). That this is a new approach to history is captured well in S. Schama (1991) *Dead Certainties* (New York: 1991).

[18] The concern with culture and power reflects the influence both of Michael Foucault and the Italian Marxist Antonio Gramsci. Gramscian interpretations are especially important for contemporary Italian archaeol-

The present essay incorporates a series of reflections on the complex, multilevel process of archaeological text creation and text reading. It begins with the 'Urtext' which is the site (or a similarly defined text-unit for archaeological investigation) and considers the degree to which that site-text is the result of conscious and unconscious 'authoring'. The archaeologist in that context appears as reader, but also to a degree not often acknowledged as author of the site. There follows a discussion of the role played by the archaeologist as the creator of texts aimed at various communities of readers, the roles that tradition plays in shaping the genres of these texts, and the ways that those written texts in turn reinforce traditions and power relations. The process of textual production and reception is advanced from the site itself through the publication of the excavations to popular presentations. At each level the triad of author, text and audience will form the framework of analysis.

The basic text that is intended to be the foundation of archaeological activity (the 'Urtext' to use the Germanic expression) has traditionally been the site. The history of classical archaeology in particular has been largely shaped by research at great sites. At places like Delphi, Ephesus and even Corinth the span of research is now in the one hundred year range. Even though the realities of modern archaeological finance and politics make the big dig less of a realistic research proposition for the future, it still very much serves as a model and metaphor for what classical archaeology is all about. Classical archaeologists excavate at well known ancient centers and find important and beautiful things.

Site investigation might be, at first consideration, that situation in which the archaeologist is most clearly the reader, extracting information and meaning from an entity created by many years of intentional and unintentional human actions and natural processes. The archaeologist digs, extracts, cleans, sorts, processes, analyzes, synthesizes, and publishes in a complicated process that culminates in the historical reconstruction of life at that site. Classical archaeologists, even with their relatively limited self reflexivity about what they do, would still

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ogy. See B. d'Agostino (1991) 'The Italian Perspective on Theoretical Archaeology', in *Theoretical Archaeology in Europe: the Last Three Decades*, ed. I. Hodder (London 1991) 52–64. Some of the most interesting explications in this area has come from such publications of Mark Leone's (1984) 'Interpreting Ideology in Historical Archaeology', in *Ideology, Power and Prehistory*, eds. D. Miller and C. Tilley (Cambridge: 1984) 25–35.

be willing to describe their activities as a form of reading. The patterns of material culture recovered are seen as mirroring the life of ancient society and providing keys to its reconstruction.

Critical reflection suggests that the intellectual processes may not be that easy. The first question that might be raised is who can be considered the author (or authors) of this site-text. Certainly many surviving features at any site were created by intentional human activity with the purpose of conveying messages to contemporaries and to posterity. Ceremonial centers, be they Stonehenge, Olympia, or Serpent Mound in Ohio were carefully and intentionally crafted to produce certain effects. The intended audiences were probably a complex combination of divinities, diverse groups in one's own society, and peers in other polities.[19] The building program of Augustus had a complex propagandistic message for a Roman society in rapid transition.[20] Burial complexes, be they the pyramids of Egypt or the barrows of Northern Europe, were clearly designed to convey messages about individual and familial power and prestige both to contemporaries and to future generations. In all those instances, the messages and meanings of those built 'texts' would only have validity if there was ongoing cultural continuity. The messages that they were intended to convey could only have been read by people who shared their cultural code. The only intended 'universal' readers were the divinities to whom certain temples and ceremonial centers were dedicated. Yet we would regard those too as culturally determined readers, since each society has its own particular deities.

The archaeologist as reader feels most comfortable with this type of archaeological texts, in large part because they represent a certain degree of authorial intentionality, which can be recovered through the scholarly process. In the case of a classical archaeologist investigating a site like Olympia, the scholar brings to the research a good knowledge of Greek history, Greek art, and Greek religion, which allows the various monuments recovered to be read in terms of a known cultural and historical context. A range of literary, epigraphical, and other

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[19] The importance of sites in conveying symbolic messages of power and control has been a major concern of the post-processual archaeologists. Relevant here are the essays collected in D. Miller & C. Tilley eds. (1984) *Ideology, Power and Prehistory* (Cambridge: 1984).

[20] P. Zanker (1988) *The Power of Images in the Age of Augustus* (Ann Arbor: 1988).

archaeological texts can be mustered to support any generalizations that are made.

The classical archaeologist hopes to understand the temple of Zeus at Olympia in the same way that the classical philologist understands Sophocles' *Oedipus Rex.* Ironically that vision of true understanding is based on a flawed assumption which threatens to undermine the whole enterprise. Not only have classical scholars since the nineteenth century sought to reconstruct Greek civilization in the technical sense of the word, but they have sought to create a high level of empathy between themselves and the Greeks. The Greeks have become us and we have become the Greeks. Increasingly more sophisticated classicists have seen a striking example of the New Critics bugaboo 'the affective fallacy' at work here. We are increasingly acknowledging that the 'otherness' of the Greeks from ourselves needs emphasis in classical scholarship. Unbridgeable gaps between the true meaning of past texts (if one can even talk about a true meaning) and the interpretations of a modern reader will always be there, whether the text is the *Oedipus Rex* or the Parthenon.[21]

However, the reader coming from outside a cultural system also has the advantage of being able to interpret these 'intentional' archaeological texts in very different ways from which the ancient 'authors' intended them to be read. The critically minded archaeological scholar can engage in a form of deconstruction that extracts alternative, unintended readings from the text. A modern Marxist interpretation of the pyramids will produce a text very different from the messages the fourth dynasty funerary priests intended to convey to contemporaries and to posterity. The same exercise can be applied to the Parthenon. That monument, which is often seen as one of the fountainheads of Western truth and beauty, by the same tools of analysis can be seen as an expression of tyrannical imperialism.

The second category of elements within the archaeological site text was created by a range of human activities which had

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[21] For the concept of the 'affective fallacy' see W.K. Wimsatt, Jr. and M. Beardsley (1954) 'The Affective Fallacy', in *The Verbal Icon: Studies in the Meaning of Poetry* (Lexington: 1954). There have been a number of studies of the way that contemporary concerns have shaped reconstructions of the classical past. One of the best is F. Turner (1981) *The Greek Heritage in Victorian Britain* (New Haven: 1981).

contemporary importance, but had no built-in aim of conveying permanent messages to contemporaries or to posterity. Tool making, food processing, food consumption, and a whole range of other part symbolic and part practical activities leave their impress in the material record. Archaeologists have debated long and hard about how to read this second level of material culture texts with their complex blend of accident and intentionality. The strategically placed coin or pot sherd has long been a key element in the reconstruction of site chronology. Empirical investigations focused on classification and chronology dominated archaeological research into the 1960's. The result was usually a site 'history' based on a series of neatly ordered and dated objects with no sense of social or cultural process.[22] The original generation of New Archaeologists hoped to be able to move beyond this stress on dating and classification and provide more information on the social dynamic. This was to be accomplished by research methodologies that stressed the 'close reading' of the patterned accumulation of debris at archaeological sites in hopes of producing the reconstruction of past human social and economic activity. By careful excavation, recording and interpretation of site components like activity areas, a true social archaeology could be created.[23]

Not surprisingly this relationship between human beings and their garbage has proved to be rather more complex and unpredictable than was originally predicted in the heady days of the early New Archaeology. The sherds, stone chips, and bone fragments that the archaeologist recovers can seldom be directly correlated with the activity that originally produced them. Disposal patterns of debris even within the sites of relatively simple societies proved to be more complex than first anticipated. People in the past as well as those in the present have generally not liked to live in or even near their own debris. The hut was swept clear, the dinner remains were buried in the back yard, and in towns and cities, rubbish was often carted and dumped far from areas of original use. While precise excavation and recording can provide much information about

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[22] Willey and Sabloff (1980) 130–80 provides historical perspective on that period of archaeology. A pioneering attack on the archaeological approach of the period was W.W. Taylor (1948) *A Study of Archaeology* (Menasha WI: 1948).

[23] Binford (1972); C. Renfrew (1984) *Approaches to Social Archaeology* (Cambridge MA: 1984).

changing human attitudes toward garbage and garbage disposal, a close connection between production, consumption, and disposal of material culture has often proved difficult to determine. Not for the first time in textual study has what initial reading saw as a finely honed work full of meaning proved to be garbage.[24]

This deconstructive approach to archaeological site meaning has not only made text reading more complicated, but also has produced a rather different text. For instance, archaeologists have too often seen architecture and artifacts as bonded together to form part of the same story. A deconstructive approach that stresses what the find spot of objects actually means makes such connections less easy. The archaeology reader has to live with more disconnections and fewer interconnections. The determination of cultural meaning from complicated archaeological texts does not prove to be that easy.

The New Archaeology view of the site as a text filled with meaning relied heavily on the assumption that much of what was studied resulted from direct or indirect human activity. However, this was hardly ever the case. Even while inhabited, the disposal patterns of the human material culture were being altered by natural, animal, and unintentional human actions that distorted or destroyed their informational value for the future archaeologist. These processes accelerated as the site is abandoned by its original inhabitants. Nature and other human groups consciously and unconsciously changed the site over time and disrupted and destroyed more and more of the patterns that would provide meaningful material cultural insights into the past. Michael Schiffer called attention to these concerns early in the development of New Archaeology and they have proved to be a major thorn in the side of the early orthodox adherents of that movement.[25]

The end result of this convoluted combination of human and natural processes has been the creation of the basic archaeological text with its complex combination of distortion and meaning. Here enters the archaeologist as reader, whose

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[24] The whole question of the relation between garbage and society is pursued in W.L. Rathje and C. Murphy (1992) *Rubbish! The Archaeology of Garbage* (New York: 1992).

[25] M. Schiffer (1976) *Behavioral Archaeology* (New York: 1976); L. Patrick (1985) 'Is There an Archaeological Record', in *Advances in Archaeological Method and Theory* 8, ed. M. Schiffer (New York: 1985) 27–62.

purpose is to create a new set of text intended for various audiences. However, the process is again rather much more complex. In reality, the archaeologist not only selects the site to be investigated from a much larger universe of potential sites, but by shaping the program of investigation determines what the site will be in the final, published record. At this juncture the archaeologist becomes not only the reader of the site but also from the beginning of field research as much as from the moment of writing the first report, the author of the site.

An example from Mayan archaeology should make this point clear. It is a well known fact that, for a long time in the history of Mayan archaeology, the great Maya sites were regarded as ceremonial centers with limited populations. They were not considered true cities. Only with the application of systematic survey techniques in Mayan archaeology did the urban size and complexity of these 'ceremonial centers' become obvious.[26]

Classical archaeologists have 'authored' their sites in a whole range of complex ways. While they have never denied the complex urban nature of most Greek and Roman sites, they have through excavation emphasis recreated an urban world largely focused around religious and political ceremonial centers. Far more archaeological efforts have gone into extracting information about temples, theatres, stadia, fora, and agoras than about houses, shops and farmsteads. Comparison of the known size of major archaeological sites, the limited areas actually excavated, and the degree to which those excavations have focused on ceremonial structures supports the assertion that the archaeologist is as much the author of the site as we see it, rather than gentle reader of what was there. Most classical archaeologists operate from the conscious or subconscious vision of a Greece where noble souls debated great thoughts surrounded by elegant marble structures rather than the probable Mediterranean reality of crowded streets, noisy shops, and vociferous and often unpleasant inhabitants.

This authoring has continued as the classical archaeologist has defined the city in relation to its rural hinterland. Most ancient cities-states were small places, where the city core blended with an agricultural suburban zone and the surrounding agricultural areas where the bulk of the population lived. Most

[26] J.E. Hardoy (1973) *Pre-Columbian Cities* (New York: 1973); W. Folen & E.R. Kintz (1983) *Coba, a classic Maya metropolis* (New York: 1983); J. Sabloff (1989) *The Cities of Ancient Mexico* (New York: 1989).

ancient cities were essentially rural central places. However, the average academic intellectual including the classical archaeologist has been an urban person with little understanding of or interest in the countryside. The result has been, until recently, the creation of a text that neglects not only the urban poor, but also rural life in general.[27]

'Time authoring' has produced serious distortions in the reconstruction of classical site history. Most Greek and Roman urban centers had long and often complicated occupation histories which continued long after the end of classical civilization. These later periods are as much part of the site history as the Roman, Greek, and earlier eras. Yet classical archaeologists have generally operated from conscious or unconscious notions of cultural superiority in which classical period Greeks placed first, later and earlier Greeks second, Romans third and the rest trailed far behind. Until relatively recently classical archaeologists in pursuit of canonical goals such as the recovery of Greek architecture and sculpture routinely sacrificed evidence for Turkish, Byzantine, and even Roman occupation and created a purified, if rather two dimensional story. Contemporary archaeological practice on the whole operates in a rather more responsible manner. Careful excavation and recording do preserve much more information on the longtime occupation history of the site. However, in their heart of hearts most classical archaeologists are still seeking the same goals as their predecessors. They want to write a Greek and Roman archaeological text.[28]

The previous paragraphs have made the argument that the archaeologist, classical and otherwise, by formulating an excavation and field excavation strategy becomes a type of author. Until the archaeologist moves onto the scene the site is mute, and the archaeologist by selection, organization, and emphasis gives it form and meaning. No archaeologist can be a simple

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[27] Recent research in both Greek and Roman studies has attempted to correct this imbalance. Selected relevant studies are R. Osborne (1987) *Classical Landscape with Figures: the Ancient Greek City and its Countryside* (Dobbs Ferry NY: 1987) and T.W. Potter (1979) *The Changing Landscape of South Etruria* (New York: 1979).

[28] This can be seen in the scanty publication of the post-Roman history of many sites. Two recent correctives to this classical emphasis at Mediterranean urban sites have been C. Foss (1976) *Byzantine and Turkish Sardis* (Cambridge MA: 1976) and C. Foss (1979) *Ephesus after Antiquity* (Cambridge: 1979).

reader, for the text does not really exist until he or she begins field research.

Yet the archaeologist also operates from the beginning as a reader, subject to all of the categories that modern reader response criticism has determined as important for understanding that process. But it does not seem useful to pursue here such topics as the psychology and psychohistory of the reader, especially given the lack of good autobiographical literature in the field of classical archaeology.[29]

One aspect of reader formation criticism which does seem very relevant is the currently much debated topic of canon formation. Canon formation scholarship has increasingly centered on the question of how we decide what is the literature of high culture worthy of serious reading and critical reflection.[30] The formulation of archaeological research programs in classical archaeology and the selection of sites for serious investigation have, I would argue, long been shaped by a canon of classical archaeological values.

The classical archaeological reading community, like most literary reading communities, was long elitist and selected its texts accordingly. In the same way that there was a canon of literary texts (mainly by white males working in the Western tradition), which was supposed to represent civilized literary production, so there were canonical archaeological sites which deserved to dominate classical archaeological discourse. Mycenae, the Athenian Acropolis, and the Athenian Agora were studied alongside Homer, Sophocles, and Aristophanes. Architecture and sculpture were more worthy of archaeological attention than the terracotta devotional figurines of popular religion, in the same way that an English literary scholar would emphasize the importance of the Brontes and Wordsworth over short stories in the Victorian penny press.

Contemporary canon debate emphasizes not only ways that the formation of the canon reflects elite taste, but also how it

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[29] D. Bleich (1980) 'Epistemological Assumptions in the Study of Response', in *Reader-response Criticism: from Formalism to Post-Structuralism*, ed. J.P. Tompkins (Baltimore: 1980) 134–63. It should be noted that the field of classical archaeology is almost devoid of good recent autobiographical and memoir literature which might aid in the understanding of the scholarly thinking of major field workers.

[30] The literature on this topic is vast. A useful introduction is J. Gorak (1991) *The Making of the Modern Canon* (London: 1991) and J. Guillory (1993) *Cultural Capital: the Problem of Literary Canon Formation* (Chicago: 1993).

is shaped by such factors as colonialism, class oppression and sexism.[31] This same socio-historical debate can be applied to the classical archaeological canon, providing many instances of ways in which research has been shaped by other factors than just the shared community values of an old-fashioned elite. Modern Italian archaeology presents illustrative examples. Italian classical archaeology has from the Renaissance been shaped by larger cultural and political battles.[32] During the fascist period, the shapers of culture, including the classical archaeological community, placed great emphasis on a glorified imperial Romanitas, which provided historical support to a dictatorial government at home and an aggressive colonial policy abroad. In the city of Rome itself, the only archaeological texts worthy of study were the architectural and sculptural monuments of imperial Rome. All else was sacrificed to the greater elucidation and more effective presentation of those monuments. The medieval urban fabric which surrounded them and represented an important part of their later history was destroyed and the pottery used by the common man (and woman) was carted off by the truck load to be dumped in the Tiber. In modern Italian colonies like Libya, excavation at sites such as Lepcis Magna emphasized the benefits of Roman colonial rule to the indigenous populations.[33]

The complex socio-cultural changes that have taken place in Italy since the fall of fascism are also reflected in the formation of a new archaeological canon. With the growing importance by the 1970's of Marxist archaeological intellectuals like Andrea Carandini, the study of economic process and the life of the suppressed underclass shaped archaeological research. An archaeological version of the study of popular fiction for the Marxist cultural historian, this new canon provided a series of different texts that were worthy of serious study. The Roman villa at Sette Finestre with its evidence for the economic base of society and for the slave mode of production replaced the Forum of Augustus as a canonical text, while even within old-

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[31] P.P. Schweikart (1989) 'Reading Ourselves: Toward a Feminist Theory of Reading', in *Speaking of Gender*, ed. E. Schowalter (New York: 1989) 17–44; G. Spivak (1986) 'Imperialism and Sexual Difference', *Oxford Literary Review* 8 (1986) 225–40.

[32] C. Springer (1987) *The Marble Wilderness* (Cambridge: 1987).

[33] D. Manacorda (1982) 'Per una indagine sull'archeologia italiana durante il ventennio fascista', *Archeologia medievale* 9 (1982) 443–54; D. Manacorda (1985) *L'archeologia del piccone* (Rome: 1985).

fashioned canonical texts newly sensitized readers provided emphases. The slave quarters of an elite house in Rome were highlighted in the same way as the shadow of colonialism in the novels of Jane Austin.[34]

It is a truism that archaeology even at its best is a science of systematic destruction. Only the most crazed of literary scholars would think of destroying the text in the process of reading. Deconstruction is a metaphorical concept applied to literary criticism. Yet literal deconstruction is exactly what the archaeologist normally does. Most contemporary archaeologists have heeded the admonition that no excavation should destroy the totality of a site under investigation, since future archaeologists will certainly be able to investigate it with greater conceptual and methodological sophistication. Yet archaeologists do destroy large portions of the site, and the realities of contemporary archaeological research are such that in few instances will archaeologists return to reread a site. In most cases the excavator is the ultimate in privileged readers. They do the reading as they are in the process of burning or at least shredding the manuscript.

The intellectual process of archaeological site reading is a complex one that would involve the consideration of much of modern method and theory. That is not my concern here. Rather the rest of the paper will focus on what I call the sociology of reading and how that relates to the transformation of the classical archaeologist from reader to author. The classical archaeologist that holds the center stage in this discussion is the excavation or project director as that person and role has been traditionally defined.

The popular image of the archaeologist is that of Indiana Jones, the loner making fantastic discoveries in exotic and dangerous places. In reality, the archaeological field director is closer to the C.E.O. of a company. As a reader, the archaeologist operates more as synthesizer, taking fragments of information provided in the field by trench supervisors and other assistants and in the laboratory by technical specialists and weaving them into an explanatory whole, an operational model that then theoretically shapes other field research decisions.

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[34] A. Carandini (1979) *Archeologia e cultura materiale* (Bari: 1979); A. Carandini et al. (1985) *Settefinestre: Una villa schiavistica nell' Eturira romana* (Modena: 1985). For the relation of colonialism and Jane Austin, see E. Said (1993) *Culture and Imperialism* (New York: 1993).

Only in the smallest operation can the director read much of the text directly. He or she depends on a series of partial plot summaries provided by others.

With the completion of field research, the archaeologist turns from reader to author and begins the process of creating other generations of texts which will in turn reach other groups of readers. The relation between authors, texts, and readers at this level is very interesting. The forms are as standardized and tradition bound as Japanese haiku poetry, yet have been the subject of relatively little self reflection. This lack of self reflection applies especially to questions of audience and the ways in which information derived from the reading of the Urtext of the site is turned into different types of written texts, designed to convey diverse constructions of information and interpretation to scholarly and non-scholarly communities of readers.

The ideal set of published archaeological texts falls into four categories, each with its particular genre form, authorial aim and intended audience. In the first instance the field director publishes a preliminary report which is a largely undigested, unreflexive account of what has gone on over the past seasons. It contains scattered information useful to specialists. It is usually published with caveats about anything reported or claimed being changed with the next season. The ostensible justification is to provide the wider archaeological community rapid access to important discoveries. It also serves to mark turf and lay claim to intellectual property.

A second category of text is the popular report. These are intended to reach beyond the world of scholars to the 'general public'. They assume a variety of forms and appear at a variety of times. They make take the form of an article in the *New York Times* or *Archaeology* magazine explaining to the lay audience the importance of research in progress. It may be a 'popular' book which summarizes in glossy form the results of several seasons of field research.

This category of archaeological literature has a long history. It goes back to the archaeological travel narratives and the accounts of pioneering nineteenth century excavations such as those of Layard, Schliemann, or Cesnola.[35] Its initial pur-

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[35] A.H. Layard (1853) *Discoveries in the Ruins of Nineveh and Babylon* (London: 1853); L.P. di Cesnola (1878) *Cyprus: its ancient Cities, Tombs, and Temples* (New York: 1878); H. Schliemann (1880) *Ilios: the City and Country of the Trojans* (London: 1880).

pose was to create an audience for the emerging field of archaeology among the literate bourgeoisie. The aim was partly altruistic, but also partly mercenary. Archaeology was and is expensive and a well healed, literate audience had to be created which would sustain the enterprise.

Increasingly this popular archaeological 'literature' is appearing in the form of television segments, videos, and museum shows. An implied aim of all of these texts is the increasing of public interest in archaeology and the special project in particular. Elements from fund raising to ego enhancement are all involved. This broad category of archaeological texts has attracted more attention from a generation of scholars interested in cultural criticism and the popular medium.[36]

The remaining two categories of archaeological publication represent the heart of the scholarly enterprise. They are the specialized expert report and the final scholarly synthesis provided by the archaeological field director. The latter will be discussed briefly at the end. In the canonical archaeological literature it has largely remained a Platonic ideal, present in universal abstraction but hardly ever brought into daily realization.

The perusal of any shelf of archaeological site reports shows that the specialist report is the dominant archaeological text. Years of archaeological research at Corinth, Cosa, Delphi or the Athenian Agora are represented by bound volumes of uniform color, which describe, classify, date, analyze and report on the public architecture, archaic sculpture, the lamps or the utilitarian pottery. In the course of the excavation the material recovered had been divided into well established categories of archaeological analysis and assigned to experts. The black glaze is placed in one set of hands, the Roman coins in another, and the lamps in a third. The person chosen is an expert in a particular area of material culture, who often has a very limited vision of the site as a whole or of the larger, contemporary world of archaeology. Entries in the published report describe color, size, shape, date, etc. Each category of material culture is related to similar material from other comparable sites. This remains to a large degree an exercise in Linnaean archaeology.

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[36] M. Leone (1984); K. Walsh (1990) 'The Post-Modern threat to the Archaeological Past', in *Archaeology after Structuralism*, eds. I. Bapty and T. Yates (London: 1990) 278–93.

Very little is said in those monographs about the relation of the objects under consideration to other categories of objects found during the course of excavation. The archaeological context considered is usually chronological and not social. The objects studied in one report are seldom combined with different categories of objects studied in others to create an image of past cultural process. The specialist report is a text aimed at two specific audiences. The first is the reading community of other specialists, who will be able to use the information to write their own specialized reports on finds at other sites or to produce historical accounts of the development of the particular artifact type under study. Such reports serve mainly to expand knowledge of amphora, black glaze or loom weights. Almost all of the information is kept within a rather arcane fraternity which forms the reading community. Communication may extend to other categories of specialists who need the information of these other specialists for their own specialized reports.

More important is the audience of the director who depends on the specialists' reports for the general summaries and syntheses of the excavation results that he or she is expected to produce. The Platonic ideal of excavation publication is that at some moment in time, all of the specialist reports will arrive on the desk of the excavator, who can then prepare the final, general report on the results of the excavation and their significance. Since classical archaeologists in particular still work from a strong, inductive mindset, the implied assumption is that the accumulated information from the specialist reports will make the writing of this final report a rather straightforward process. Take the reports, synthesize them, add insights and interpretations from the director's knowledge and experience, and the great, final interpretation of site history will appear.

Unfortunately this final stage in the great textual chain of being almost never appears. A rapid review of the major American classical excavations conducted since World War I shows that in almost all instances the field director did not complete a satisfactory final summary of the excavation. One has to make do with popular or relatively general scholarly works, often produced long before most of the specialist studies had been completed.[37] Part of this failure can be blamed on the

[37] Examples in this latter genre include for Cosa, F. Brown (1980) *Cosa, the Making of a Roman Town* (Ann Arbor: 1980); for Dura, C. Hopkins (1979)

complex administrative and fund raising responsibilities (plus other academic duties) which burden the director of a modern excavation. However, blame can also be placed on the overwhelming information flow of a modern excavation that swamps the director, contributing at times to a form of intellectual paralysis in a scholar who still tends to work in a largely inductive mode. Most contemporary classical archaeologists are trained as cautious positivists, whose greatest fear in life is making some unsubstantiated statement which will be refuted by the next potsherd produced in trench 1A, and will then expose them to professional ridicule and humiliation.

Part of the problem results from the complex power interactions between the authors creating different levels of archaeological texts. The director, like an editor in complex literary projects such as the More or Walpole papers, exercises tremendous power by his ability to assign desirable collections of documents to underlings. Once the material has been assigned and the detailed research is under way, the specialized author is much more in a position to exercise power by holding the director hostage. Many specialists find little incentive to finish their reports, thus depriving the central archaeological author of key information that is regarded as essential for the completion of the grand synthesis.

The outcome is that most directors die without producing the magnum opus which summarizes their life work at the archaeological site (or in some cases sites). A large part of the initial text of the site which had been destroyed by this excavation and is supposed to be recreated in the mind and ultimately the writings of the chief archaeologist disappears with him. Clearly the failure of most excavators to provide that grand synthesis is a serious blow to archaeology. Not only is much raw information lost, but also the thoughts and insights of the scholars who did the work are lost. Few archaeologists leave diaries or publish autobiographies. All that survive are the bones and sherds of archaeological research in the form of notebooks, catalogue cards and record photographs. With the intended interconnections lost they remain, like the original sherds and bones, largely mute.

Clearly new modes of expression are required if the text of

The Discovery of Dura Europos (New Haven: 1979); and for Sardis, G. Hanfmann (1972) *Letters from Sardis* (Cambridge MA: 1972) and G. Hanfmann (1983) *Sardis from Prehistoric to Roman Times* (Cambridge MA: 1983).

the site is going to be turned into any meaningful written text suitable for sophisticated archaeological discourse. In part, the archaeological director has to see him/herself as more than an inductive scientist and has to produce more diverse types of meaningful if not definitive texts. One can argue that the creation of any degree of real humanistic meaning in the fragmented, disconnected world produced by field archaeology requires the imaginative insight of the historical novelist, who weaves together the limited physical evidence of the historical record with fictive but probable connectors. Post modern historiography is moving in a direction which bridges the traditional worlds of the fiction writer and the historian. While one may be reluctant to advance the 'any approach if it feels good' stand of some Post-Processualists, still one can argue the need for the creation of a new genre of texts in which authors of authority (i.e. field and project directors) are encouraged to lay out their views and insights on a site, using the evidence responsibly, but not being afraid to take intellectual risks.[38] The audience would not be the layman, but scholars who would appreciate the insight of thinking archaeologists into past social process as much as their accounts of the square centimeter size of certain potsherds.

It is rather sad to end an essay on archaeology and texts with elegiac laments for great texts that were never written. Some readers will regard the discourse of the previous pages as the slightly absurd efforts of a middle aged archaeologist to be trendy, while he could better be spending his time cataloging more Roman utilitarian pottery. However, at the time when a major law school considers the theories of a Stanley Fish sufficiently relevant to incorporate him in its faculty, classical archaeology cannot afford to ignore these questions of discourse and communication. In spite of the title of a popular book on Roman archaeology, the 'mute stones' do not speak. They are at best texts that must be read and then turned into a series of texts that are read again. Meaning is created, conveyed, and distorted only through that process. It is important that we reflect on it and bring to that reflection the best of contemporary method and theory.

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[38] M. Shanks (1992) *Experiencing the Past: On the Character of Archaeology* (London: 1992).

THUCYDIDES AND THE BEGINNINGS OF ARCHAEOLOGY

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INTRODUCTION

Archaeology, understood as a discipline (or subdiscipline) is about as old as anthropology, but it is very young in relation to history. In the long history of history writing, archaeology and history have only recently been distinguished: traditionally both modern fields have been subsumed in one pursuit. Historians, in other words, have traditionally concerned themselves with both words and objects as evidence for the past.[1] The problems involved in the interpretation of each, and the relationship between the two have occupied western historiography since the time of the earliest Greek historians, Herodotus and Thucydides, in the fifth century B.C. Clearly enough, there was nothing new or distinctive about the interpretation of objects per se, even in the fifth century B.C. People, for at least as long as they have been using tools, have always been obliged to subject material objects to complex and ordered interpretations in the simple context of their daily lived experience.[2] As I will argue, however, archaeological interpretation

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[1] The connection between prehistoric archaeology and anthropology is as old as the discipline of anthropology itself: see e.g. B. Trigger (1989) *A History of Archaeological Thought* (Cambridge: 1989) 110–47 and passim. For the disciplinary definition of archaeology as it has developed in Europe and America, idem (1978) *Time and Traditions: Essays in Archaeological Interpretation* (New York: 1978) 75–95. On the use of objects by historians, David Lowenthal (1989) *The Past is a Foreign Country* (Cambridge: 1989).

[2] The role of things in the structuring of cognitive processes has long been recognized. For Greece, see e.g. G. Nagy (1990) 'Sema and noesis: the hero's tomb and the reading of symbols in Homer and Hesiod', in his *Greek Mythology and Poetics*' (Ithaca: 1990) 202–22. Even more generally, compare Bourdieu's notion of habitus and its role in the development of the person: Bourdieu (1977) *Outline of a Theory of Practice* (Cambridge: 1977). The role of things, particularly of tools, in human cognitive development has often been emphasized, perhaps most notoriously in Soviet psychology: see C. Ratner (1991) *Vygotsky's Sociohistorical Psychology and its Contemporary Applications*, (*Cognition and Language: A Series in Psycholinguistics*)

of objects is set apart from this common, quotidian understanding by its insistence on fixing a historical meaning, separating the object from the mutable meanings attached to it through daily use and contact. In short, the salient feature of archaeological interpretation is that it regards things as a kind of writing, a hybrid, which combines certain commonly recognized qualities of objects with those of words. So conceived, it becomes possible for objects to communicate originary meanings independent of the fluctuating, evanescent, living consciousness of a community.

Only ten years ago Glyn Daniel remarked (in a rather casual and provocative way) that 'archaeology is that branch of the study of history which deals with the material remains of man's past. The study of history in its widest sense deals with all sources . . . The archaeologist deals with man's artifacts.'[3] The connection with history, so long observed in the conceptual presuppositions and interpretive goals of archaeology (even when not in the institutional and disciplinary affiliations of professional academic archaeologists) was at the time and has remained anything but unproblematic. The broader distinction which Daniel is making between the study of words and the study of things, however, seems unexceptionable to me: as a discriminable field of inquiry, archaeology can be distinguished from others, such as history or anthropology, by the material (as opposed to the verbal) character of its sources.

In recent years problems pertaining to the disciplinary status of archaeology, such as its own history and theoretical underpinnings, have increasingly preoccupied archaeologists.[4] In discussion of these issues the dichotomy of words versus things, whether explicitly acknowledged or not, has loomed large. For example, it is often said that there are two kinds of archaeology: prehistoric and historic. Prehistoric archaeology, the archaeology of societies which are not documented by writing

(1991) 47–58; cf. further D. Joravsky (1989) *Russian Psychology: a Critical History* (Oxford: 1989).

3 *A Short History of Archaeology* (London: 1981) 13.

4 The work of Glyn Daniel provides a useful introduction to the topic: see e.g. the convenient summary provided in the popular and relatively recent Daniel (1981). The essay of G.R. Willey and J.A. Sabloff (1974) *A History of American Archaeology* (London: 1974) is still useful. For more recent developments and controversies as well as thorough bibliography see e.g. Trigger (1989); J. Malina and Z. Vasicek (1990) *Archaeology Yesterday and Today: the Development of Archaeology in the Sciences and Humanities* (Cambridge: 1990).

or other verbal sources, has typically been formally associated with anthropology, although thinking about many of the traditional problems and themes of prehistory has been influenced, has even originated out of the conceptual framework of historical investigation. Meanwhile, the archaeological investigation of literate societies (such as Greece and Rome) has often been conducted under the aegis of history.[5] The professional and intellectual affinities of archaeology with these two disciplines has been a battleground particularly since the 1960's when certain archaeologists rejected traditional historical concerns as a legitimate program of research in favor of a positivist, 'scientistic' anthropology.[6] Others since have argued for a recuperation of history and a more hermeneutical (one might even say literary) approach to archaeology.[7]

At the heart of these discussions about the nature and status of archaeology is a basic discrimination regarding sources, that broad and age-old distinction between words and things. This issue has become increasingly apparent in the discussions of recent years. In particular there has been a growing concern with 'archaeology as archaeology', with the special, distinctive characteristics and problems which the interpretation of things involves. Increasingly it has been argued that the material sources of archaeology pose problems of interpretation and provide kinds of information, answer questions, which differ qualitatively from the issues commonly regarded as important by historians or anthropologists, who have access to verbal accounts

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[5] Furthermore, as Bruce Trigger has pointed out, archaeology tends to be connected with anthropology in America and history in Europe: see Trigger (1978) 75–95. For the development and disciplinary status of classical archaeology see Ian Morris (1994) 'Archaeologies of Greece', in *Classical Greece: Ancient Histories and Modern Archaeologies*, ed. I. Morris (Cambridge: 1994). Morris argues that Greek archaeology was absorbed into the Classics primarily because of the latter's prestige.

[6] These claims are of course associated above all with Lewis Binford and the 'new archaeology': see e.g. L.R. Binford, (1962) 'Archaeology as anthropology', *American Antiquity* 28 (1962) 217–25; S.R. and L.R. Binford (1968) *New Perspectives in Archaeology* (Chicago: 1968); D. Clarke (1978 *Analytical Archaeology*, 2nd ed. rev. by Bob Chapman (New York: 1978) and the review by S. Shennan (1989) 'Archaeology as archaeology or anthropology? Clarke's *Analytical Archaeology* and the Binfords' *New Perspectives in Archaeology* 21 years on', *Antiquity* 63 (1989) 831–35.

[7] So especially the 'post-processual' archaeologists: see e.g. I. Hodder (1991) *Reading the Past: Current Approaches to Interpretation in Archaeology*, 2nd ed. (Cambridge: 1991). For some notable applications see C. Tilley (1991) *Material Culture and Text: the Art of Ambiguity* (London: 1991); J. Thomas (1991) *Rethinking the Neolithic* (Cambridge: 1991), particularly the chapter on 'Reading monuments', 29–55.

of various kinds.[8] The controversy as to whether archaeology should be considered as an accessory of history or anthropology, some would argue, is misguided; archaeology should rather be cut loose from both and regarded as an autonomous discipline.

In his introduction to this volume, David Small sums up and criticizes a common, even anti-archaeological conception of the relationship between words and things. As he notes, many have imagined that 'the textual record is a frame, in other words, the principal source, against which archaeology plays a secondary role'.[9] This attitude regarding sources has important implications for the disciplinary character of archaeology as well. Archaeology is often imagined as the 'handmaiden of history', or of anthropology, or of some other field which has access to verbal texts; it is an 'ancillary' discipline, because it is commonly held that things can only supplement words, fill in the blanks for which the written record provides the frame. While material objects may sometimes provide useful addenda to this received written record of facts, they are not suitable or adequate in and of themselves to serve as the sole or primary evidence supporting any investigation of the past or present. This position (or as some would have it, prejudice) raises difficult problems for archaeologists. What does material culture mean? Can it mean anything outside of linguistic explanation? Or is all meaning bound up in words? This problem is too deeply ingrained in the traditions and habits of thought of western culture and in the practices of archaeology to be facilely dismissed, even by archaeologists who insist on the autonomy and sufficiency of material meaning.[10] After all, archaeologists do not simply show objects; they provide verbal accounts of objects. In fact, it is arguable that the unique goal of archaeology is to provide verbal exegesis of material culture, to give voice to mute things, to provide that 'textual frame' within which the thing can be understood.

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[8] For a convenient review of this discussion, see Trigger (1989) 357–69. For the hostile reaction of an historian, see M.I. Finley's criticisms of David Clarke in his article (1975) 'Archaeology and history' in his *The Use and Abuse of History* (New York: 1975) 87–101, esp. 91–93.

[9] Small, intro., p. 4.

[10] Bruce Trigger (1989) 72, notes briefly that dependence on words is a 'restricting assumption' from which antiquarians were liberated by the creation of prehistoric archaeology. So it might be said that modern archaeology does not begin until it is possible to divorce words and things.

Here, as elsewhere, things do not speak for themselves: someone must speak for them.

On the other hand, despite this subsidiary role which is often allocated to material culture, there is simultaneously a sense in which things are given pre-eminence over verbal sources. Things, unlike words, appear to be true and real, not subject to the vagaries of ideology. Words are referential; they refer to things other than themselves. Their representations can be unreliable, biased, incomplete, or simply false—the contrary of the thing. Things are solid and tangible, monumental: they are there, referring only to themselves, the truth of which their very presence, their concreteness, guarantees. By the simple fact of their being there, the fact that they are, they seem indisputably real and true as words do not, and so can be used to verify the shifty, ambiguous representations of the verbal record. This prestige of things is not necessarily an advantage for archaeologists. Many of the methodological problems and practical aporias which archaeology currently wrestles with stem from the apparent reality of the thing. Archaeologists would like to be able to read the object as a kind of text, but in order to do so they must constantly struggle against the apparent reality of the object. This reality is mute and incommunicative, but it confers a prestige which few are willing to give up.

This simultaneous dependence and preeminence of things to words springs from a peculiarly Greek vision. In this paper, I will be concerned to describe the extraordinarily different attitudes of the two earliest surviving Greek historians, Herodotus and Thucydides, toward the use of things as sources, and how they imagine the relationship of these things to words. At the same time, I will explain some of the social and cultural changes which underly the conceptual differences between the two. As will be seen, Herodotus does not admit that things can have meaning outside of verbal tradition, though he does think that they can serve to preserve and verify that tradition. Thucydides, on the other hand, is able to imagine that things can have meaning independent of what is said of them, and argues for a specifically utilitarian logic to this meaning. In this attitude, he might well be described as the first Greek archaeologist. This retrospective of the shared origins and development of western history and archaeology should also shed some light on the current discussion of the prospective relationship and goals of the two.

WORDS AS THINGS: PLATO ON WRITING

Plato writes that Socrates and Phaedrus went for a walk one sweltering morning outside the city walls of Athens, down by the Ilissos river. They paused in the shade of a plane tree near a shrine of the muses, the goddesses of memory, and talked, among other things, of the nature of speech, writing and memory. Toward the end of the discussion, Socrates told a story of how 'the father of letters,' a certain Egyptian named Theuth, recommended his invention of written language to the king of Egypt, Thamus. Theuth believes that writing will 'make the Egyptians wiser and improve their memories; for [it is] a drug of memory and wisdom'. The king of Egypt is unimpressed. He responds that writing will actually produce the opposite effect from that which its inventor imagines. This discovery, he says,

> will produce forgetfullness in the minds of those who learn how to use it, due to the neglect of memory. Because of their trust in writing, produced by alien characters from without, they will not remember of themselves, from within.

In writing, the king charges, Theuth has invented a device 'not for memory, but for reminding' (*Phaedrus* 274 E-275 A).

Plato has Socrates attribute to the king of Egypt an attitude which is typical of much of western philosophical and metaphysical thought. Writing, as opposed to speech, has often been conceived as a defective, secondary form of representation:[11] unresponsive, distant, alienated from human thought, it seems opaque and inscrutable. What it communicates is oddly divorced from the agency of a communicator, a speaker. Information preserved in writing cannot be described as 'kept in memory,' for, separated as it is from human agency, there can be no memory associated with writing. Speech, in contrast, has an apparent immediacy, which seems to be lacking in writing; in the days before the electronic media, it could emanate only from some obvious and present being - a human consciousness and memory.

Because of its disengagement from its author, or even from

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[11] In recent years the point is commonly associated with J. Derrida: see (1976) *Of Grammatology* (Baltimore: 1976); idem (1982) *Dissemination* (Chicago: 1982). For a quick review of Derrida's thought on writing, see J. Culler (1982) *On Deconstruction: Theory and Criticism after Structuralism* (Ithaca: 1982) 89–110. For other bibliography on the Phaedrus and Plato's conception of writing see e.g. J. Neel (1988) *Plato, Derrida and Writing* (Carbondale: 1988).

any author, writing is by nature unclear, and vulnerable to abuse by those who wish to use it for their own ends. Unlike oral exchange, which is dynamic and mutable, writing is static and unchanging. In conversation, a speaker can change and adjust an argument to suit any circumstances or meet direct objections. Once words are committed to letters, however, they are fixed forever: they cannot respond to new problems or situations. They simply continue mindlessly to parrot the same phrases over and over. As Plato writes that Socrates says, 'writing is very much like painting': creatures outlined in this fashion look as though they were alive, but when they are questioned they can only maintain their same dull and solemn silence.

> It is the same with [written] words; you might imagine that they spoke as if they had some intelligence themselves, but if you question them, wishing to learn, about what they say, they indicate always only one and the same thing. When once it is written, every word is rolled about everywhere, both among those who understand and likewise among those who have no interest in it; it has no comprehension to speak to whom it should and to whom it should not. When mistreated or unjustly abused it always needs the help of its father, for it has no power to defend or help itself (*Phaedrus* 275 D-E).

Perhaps more than any other characteristic, it is the materiality of writing which is responsible for these attitudes. Speech is communicated through sound: it is invisible, evanescent. It effaces itself in the moment of hearing or utterance: no sooner heard or spoken than gone. This immateriality and impermanence contributes to an association, even an indiscriminacy, of word and thought. To the one speaking, it may appear that speech and thought are fused in the internal moment of conception; to the one hearing, it may be difficult to distinguish between the sound and the meaning it evokes in the instant of apprehension. The written word, on the other hand, is accessible through sight; committed to letters, it becomes physical and lasting. Unlike speech, writing survives its creation and decipherment, outlasts the moment of its comprehension, and so endures as a marker, or reminder, of the separateness of word and thought.

The permanence of writing is disconcerting and troubling because it alienates that most human of characteristics, language, from the human mind. Writing removes meaning from the interior, sentient realm of consciousness into the external, brute, inanimate world of things. At the same time, how-

ever, the physical character of writing can be comforting. The spoken word is evanescent; it cannot be seen, smelled or touched. It has no duration, except in memory - and memory, too, is notoriously fleeting and changeable. Speech and memory, in a way, are unreal - at least they are not real in the sense that concrete material things are real. Writing, as it partakes in the physical nature of things, offers a remedy for these failings of oral speech and memory. Tangible, enduring, visible: it is there (and so real) as speech is not.

Although (or perhaps because) it is clearly distinct from thought, writing can serve to preserve memory. This autonomy poses a threat as well: there is a danger that writing can be used as a replacement for memory. Plato's Socrates acknowledges certain benefits, but he nevertheless insists that writing must not be allowed to supplant memory, that true knowledge can only be kept in consciousness, and cannot be preserved in residual form. A wise man - such as even a Socrates himself might pray to become - must realize that

> in the written word there is necessarily much that is playful, and that no argument has ever been written, whether in meter or in prose, which deserves to be treated very seriously... but truly the best of them are a reminder of what is already known (*Phaedrus* 277 E - 278 A).

This conclusion, that writing should be used as a reminder, as a prompt or supplement to memory, rather than as a substitute for memory, is repeated several times in the *Phaedrus* (cf. 275 A; 275 C-D; 276 D). To paraphrase, writing has value when it is understood as an aid, and nothing more, to human consciousness. The only good writing is an unread writing; it is dangerous when it is taken to be something intelligible in and of itself. Writing should not be used as a repository or source of information, but as a kind of mnemonic device. The physical word should ideally serve as a marker which summons up information which everyone already has in mind.

By a similar reasoning, it might be argued that writing can serve as a verification as well. Memory is ephemeral and corruptible: over time it is unreliable. The written word cannot replace memory, because it is unresponsive and unable to justify itself upon examination. For this reason, writing cannot represent anything true, unless it is supplemented by memory. Being concrete and enduring, however, it may serve as a check against which the accuracy of memory may be gauged. In the

case of the law, for instance, this is not a negligible function. Unwritten, standards of social conduct are subject to perversion and abuse. Once written down, they are fixed and so can serve equally as an appeal and resort where anyone who desires can verify them. Again, however, written codes cannot serve as an autonomous, external standard of conduct, which acts on people independent of their volition and knowledge - this would be the destruction of law. For law to be valid and effective, it must be kept consensually by individuals in heart and mind. Only once rules of conduct have been internalized, when everyone already knows them, can law be said to exist; writing should only be used to provide a check, or verification of this preexisting knowledge.[12]

Plato's Socrates does not emphasize this verificatory function of writing, though the point is entirely consonant with the argument of the *Phaedrus.* As Socrates notes, the great flaw of writing is that it cannot represent the truth: with this point the *Phaedrus* concludes (cf. particularly 277 C - 278 C). The problem is that writing, by virtue of its materiality, its apparent reality, has an authority and presence which is lacking in speech. Because it is tangible (and so real), the written word appears to be true - falsely.

> [Go and tell] Solon and whoever has written compositions in political terms, calling them laws. If he composed these with knowledge of the truth, and is able to go to their aid by discussing what he has written, and is able to show by his speaking that what is written is trivial, such a man should not receive his title from such writings, but from those discussions to which he has devoted serious work (*Phaedrus* 278 C).

To elaborate on this line of reasoning, we might say that written words in themselves cannot express truth, which resides in living discussion, the give and take of argument and cross examination. Rather it can only serve as a reminder of this other, pre-written truth - as a verification in other words.

There are obvious similarities between the reminder and the verification. The most significant of these is one of perspective, specifically sequence. The reminder precedes memory,

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[12] I have made this point briefly in my article (1993) on 'Democracy and literacy', *The Birth of Democracy: An exhibition Celebrating the 2500th Anniversary of Democracy* eds. J. Ober and C. Hedrick (Princeton: 1993) 7–11. A longer version of this article will appear in a festschrift for D. Lewis under the title 'How to not read a democratic inscription.'

and is its prompt. The verification follows memory, and is its proof. The same thing obviously can in any instance serve either as reminder and verification, or as both, depending on the relationship imagined with the memory it verifies or calls to mind. Both proof and prompt are related to memory as contingencies, supplements: they are simultaneously aids, whose existence is dependent upon memory, and guarantees, which have a priority over memory.

It has often been remarked that Plato's philosophical position on writing is linked to the social and cultural attitudes of Greece in the fourth century B.C.; his critique of writing is informed by a mentality which is itself the product of written culture.[13] In a banal way, it might be said that the *Phaedrus* is an essay written against writing, and so is obviously a product of the literacy which it criticizes. More significantly, however, the arguments and reasoning, even the style, which Plato employs are all indicative of a literate author. Plato, unlike his mentor and protagonist Socrates, is above all a writer. It must be emphasized as well, however, that Plato's arguments are at the same time informed by the practices and ideals of a contemporary society in which literacy was not widespread. In particular, as will be seen, the position that the written word should not be independent of memory, but should serve to remind and verify, is typical of a certain practice of this other, unlettered society.

THINGS AND WORDS

The world is full of material objects, alien soulless things which appear to exist and endure independent of human apprehension and consciousness. At the same time, however, they are inanimate. The idea that a meaning resides within them, that they could somehow communicate that meaning without a human intervention, is strange, counter-intuitive - even threatening. Certainly all things are the constant objects of human interpretation and understanding; but they are passive objects. Unlike written words, they are unable to speak on their own:

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[13] The classic essay on the place of Plato in the Greek 'literate revolution' is by Eric Havelock (1963) *Preface to Plato* (Cambridge: 1963). Much has been written since, by Havelock and others. See the reviews in R. Thomas (1989) *Oral Tradition and Written Record in Classical Athens* (Cambridge: 1989) 15–38; eadem (1992) *Orality and Literacy in Ancient Greece* (Cambridge: 1992); T. Lentz (1989) *Orality and Literacy in Hellenic Greece* (Carbondale: 1989).

when questioned, they cannot even say 'always the same'. They are mute and can only 'preserve a solemn silence'. Intransigent and intransitive, this silence communicates above all the presence of the object: 'I am here'.[14] Things, then, cannot of themselves speak anything beyond themselves to anyone - they cannot tell a story to someone who has not heard it before. They can, however, by virtue of their permanence, serve to remind those who already know. Things provide no access to the unknown; they can, however, by virtue of their simple, material reality, the truth of their incontestable presence, serve to verify what is known.

The use of things to preserve and prove the past is one of the characteristic practices of Greco-Roman civilization. The Greeks used a variety of words, some more, some less specific, to describe these objects: of these, a notable, generic term was μνῆμα, which might be translated as 'monument' or, very literally, as 'reminder'. These objects might be found in a variety of contexts, but particularly as dedications, which were deposited in local shrines and other public places. These dedications were of various kinds: they might be large or small, permanent or portable, aniconic or figurative, natural or fabricated.[15] Anything can serve as a reminder, even a rock or a tree, provided only that its story is already known. As Plato has Socrates remark to Phaedrus (with some humor):

> My friend, some claimed that the words of the oak in the sanctuary of Zeus at Dodona were the first prophetic utterances. Because of their simplicity, it was enough for the folk of that time, since they were not as wise as you youngsters, to hear an oak or a rock, provided only they spoke the truth; but to you perhaps it matters who is talking and where he comes from, for you do not consider only whether his words are true or not (*Phaedrus* 275 B-C).

Things can only serve as reminders, however, if they are

[14] Roland Barthes' short essay (1988), 'The Semantics of the object', reprinted in *The Semiotic Challenge*, trans. by R. Howard (New York: 1988) (60–69), is particularly good on these 'existential' connotations of the object. See also R. Jacobson (1987) 'On the relation between visual and auditory signs', reprinted in *Language in Literature*, eds. K. Pomorska and S. Rudy (Harvard: 1987) (33–51). For further thoughts about words and things see Stephen A. Tyler (1987) *The Unspeakable: Discourse, Dialogue and Rhetoric in the Postmodern World* (Madison WI: 1987).

[15] The major review of this class of object remains the old essay by W.H.D. Rouse (1902) *Greek Votive Offerings: an Essay on the History of Greek Religion* (Cambridge: 1902).

integrated into the life of a community. So long as people see things, move around them, live with them, they will know what they mean. This meaning of things 'which everyone already knows' is obviously not a fixed meaning. The stories which are known about the ruins in the town center will fluctuate over time, vary to some greater or lesser extent from person to person. Doubtless there will be certain common features in the stories told about a given object by different informants at different times and places. The cultural meaning of monuments, however, is inconsistent, variable and subjective.

Archaeology, like history, is a typical practice of a literate mentality: its procedures are informed by the logic of writing. Its most characteristic and problematic practice is the decipherment of historical meaning from objects without the assistance of a verbal tradition. The meanings of things which it attempts to discover are originary, invariant, true meanings. Archaeology, in other words, attempts to read material objects in the same way that historians 'read' words, as though objects were a kind of writing which endures autonomous of the vagaries of local tradition and culture, that is, of the inconsistencies and impermanence of the spoken word. To make this archaeological 'reading' possible, it is necessary to postulate a vocabulary, grammar and syntax: in the modern world it has often been supposed that things speak the universal esperanto of utility. So, as Gordon Childe claimed, the 'science' of archaeology is founded on the discrimination and classification of objects as tools, that is, of objects in their manifestations as use values.[16] Once such a grammar has been posited, objects can be run through the hermeneutic meat grinder, their uniform meanings ground out of them.

Archaeology begins, not with things, but with writing. The central idea which defines and informs its practices and theories is the same as that which informs reading: that there is an accessible meaning 'out there', that is, outside of consciousness. Archaeologists seek to decipher this meaning of things. As we have seen, writing seems to remove language from human agency and consciousness: the written word appears to speak to us as though it had a will of its own. This disembodied voice, this anonymous speech, suggests that meaning is not the sole property of human interactions, but can be removed from thought and intention and located even in inanimate

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[16] V. Gordon Childe (1947) *History* (*Past and Present* 6) (London: 1947) 6.

objects, things. Writing is perhaps not the necessary or sole precondition for the recognition of the separation of meaning from human mind; it surely emphasizes and clarifies that separation, however. It provides a remorseless reminder that meaning is not indivisible from the person: it is alien, something other than thought. Once it is realized that writing can speak without the immediate sponsorship of human agency, it becomes imaginable that autonomous meaning can reside in other things as well.

HERODOTUS

Herodotus is the first of the Greek historians whose work has survived to modern times. His history, probably composed between about 440 and 425 B.C., is broadly concerned with 'the wars of the Greeks and Barbarians', and in particular with the Persian wars of 490–479 B.C. In the process of telling this story, however, he manages to recount much other information, particularly ethnographic information: the geography, history, habits and customs of the other notable peoples who inhabit the Mediterranean littoral and its hinterlands.[17]

Herodotus' history appears to have been performed aloud for groups of auditors, rather than read privately, as we today would read a book. Even so, it is clear that writing must at some point have played a role at some point in the composition of the histories - it would not otherwise have been preserved to us. Furthermore, Herodotus himself occasionally refers to his own authorial actions as 'writing'. Nevertheless, for the most part Herodotus describes his own position as author and his work in the terms of oral composition. When he himself, the author, intrudes, it is mostly to 'speak'.[18] When he refers to the history, or to its various discriminable components, he calls them λόγοι, that is, (oral) stories.[19] Certainly despite the

[17] There are many essays on the history of Herodotus. For a brief introduction see e.g. J. Gould (1989) *Herodotus* (London: 1989). For a survey of traditional problems and bibliography I would recommend C. Dewald and J. Marincola (1987) 'A Selective Introduction to Herodotean Studies', *Arethusa* 20 (1987) 9–40.

[18] On the relationship of writing and speaking in Herodotus, see above all Francois Hartog (1988) *The Mirror of Herodotus: the Representation of the Other in the Writing of History*, trans. by J. Lloyd (Berkeley: 1988) 260–309.

[19] For Herodotus' references to the internal components of his history see particularly H. Immerwahr (1966) *Form and Thought in Herodotus* (*American Philological Association Monograph Series* 23), (Cleveland: 1966) 14–15.

ambivalences of Herodotus' attitude and representations, his history is obviously a written thing. He understands this writing, however, as Plato argues 'good' writing should be conceived: the history, like a monument, is a reminder and verification.

The mutual dependence of tradition and object is clear from the outset of Herodotus' history. In the first line of the history, in the proem¡, Herodotus remarks that

> This is the verification (ἀπόδειξις) of the investigation (ἱστορίη) of Herodotus of Halikarnassos, so that the occurrences due to men (τὰ γενόμενα ἐξ ἀνθρώπων) may not become evanescent (ἐξίτηλα) in time and so that the great and marvelous monuments (ἔργα μεγάλα τε καὶ θωμαστά), some called to witness (or performed—ἀποδεχθέντα) by Greeks, some by Barbarians, should not lose their renown (literally, become ἀκλεᾶ), and specifically for what cause (αἰτίη) they fought with one another (Proem 1–4).

I have outlined a few of my opinions about this passage elsewhere.[20] Here I should like to emphasize only the one or two points which are necessary for my purposes in this essay. Herodotus never discusses the possibility of the eradication, or destruction of the realia of the past. What happened in the past may well be eternal and indestructable: whether people know of it or not, it always will have happened, its traces will continue to exist. Knowledge of the meaning of this past, however, is clearly not eternal. Herodotus is particularly concerned with the possibility of a split, a separation of the past from its oral report.[21] By means of his history, he claims to vindicate this past from the ravages of time. He specifies two varieties of raw material of the past: events (γενόμενα) and concrete objects (ἔργα).[22] Herodotus notes that the problem with events, for example, battles or love affairs, is that, like oral utterances, they are evanescent (ἐξίτηλα): no sooner have

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[20] Charles W. Hedrick, Jr. (forthcoming) 'The meaning of material culture: Herodotus, Thucydides and their Sources', in *Nomodeiktes: Greek Studies in Honor of Martin Ostwald*, eds. R. Rosen and J. Farrell (Ann Arbor: forthcoming). The Standard essay on the proem is by T. Krischer (1965) 'Herodot's Prooimion', *Hermes* 93 (1965) 159–67.

[21] This concern is not unique to Herodotus: even Homer is aware that 'objects may fail of their renown.' See G. Nagy (1990) 202–22.

[22] The current orthodoxy has it that these two terms are virtually synonymous: see H. Immerwahr (1960) 'Ergon: History as a Monument in Herodotus and Thucydides', *American Journal of Philology* 81 (1960) 261–90. See my article, (forthcoming) for reason why I disagree.

they occurred than they are vanished, to be kept only in memory. Objects (ἔργα) are obviously not evanescent as are events: They endure and outlast the death of their maker. Their meaning, however, is not secure: only through oral renown (κλέος) can they be understood, and oral renown, like any verbal account, is ephemeral and corruptible, vulnerable to time. Objects are thus at risk of being separated from the stories which are told of them, of failing of their fame, becoming ἀκλεᾶ, meaningless.

Herodotus' history is explicitly intended to rectify these failings of the received tradition. Herodotus' goal is not to correct mistakes, or uncover new events, as many modern historians would intend in his place. Rather, he is concerned to shore up memory of what is already known. It is clearly possible for events to lose contact with the traditions which explain them, and even so to continue to exist. In such a state, however, they would be meaningless, or at best, hopelessly ambiguous. Through his history, Herodotus intends to keep word and thing together, to preserve a meaning for the omnipresent material traces of past human achievement. The world is full of such traces: it is clear, by virtue of their very remarkableness, that they must mean something. Herodotus, history will sustain this meaning, which the things in and of themselves are inadequate to maintain. So, as he says, his history is intended so that 'events will not fade nor objects fail of their report'.

The attitudes which are seen in the proem are obviously informed by the mentality of writing. The history is intended to guarantee oral fame and preserve the memory of events. To do so it must be both permanent and comunicative, dividing itself between the natures of the object and the spoken word. Conceived as an oral performance, the history would be utterly vain and pointless: as fleeting and evanescent as the events and stories it is meant to preserve. In order to carry out the function which Herodotus proposes for it, the history must have the qualities of writing.

The word which Herodotus uses to describe his history in this passage is ἀπόδειξις. Herodotus uses this term to name his composition only here, in the proem. Elsewhere, as I have noted, he commonly refers to his history as a λόγος, that is, as an oral production. In the proem, however, Herodotus is dealing as explicitly as possible with the history itself as a source, as another verbal account, comparable to the many which are subsumed within it. The function of the history is to guaran-

tee oral tradition, and this it cannot do, if it is itself an oral production. Accordingly, Herodotus must set it apart as something else: something which is different in significant ways from the materials contained within it.

A variety of translations have been suggested for ἀπόδειξις, notably display or the oral equivalent of publication. I have suggested that the word be taken instead to refer to one of Herodotus' own practices of source criticism. In the history the related verb ἀποδείκνυμι has a variety of meanings.[23] One of these meanings refers to the process of verification, the appeal to a source which validates a story. In particular, in Herodotus, sources tend to be things. So, for example, when the people of Croton and Sybaris wish to prove the truth of a story, they 'show as proof' (ἀποδείκνυμι) certain objects (5.45.1–2). In the proem, Herodotus' ἀπόδειξις ἱστορίης, the verification of his inquiry, is the logical connection which allows him to tie together things (ἔργα) and oral renown (κλέος). The idea of 'verification' also describes the mediating character and function of writing for Herodotus: it combines the endurance of objects with the meaningful communication of verbal story. The meaning of the object is in the story, the proof of the story is in the object. Writings like Herodotus' history can only verify a tradition which is already known.

Herodotus' historical practices illustrate an attitude toward objects and words which is typical of and consistent with the programmatic statements which he makes in his introduction. He normally does not conceive of meaning as external, located in things autonomous of human memory and agency. For him, objects do not mean anything, except insofar as they are a part of a living oral tradition. It is not simply, however, that objects are dependent for their meaning on oral accounts. They also play an important role in fixing the tradition. They may be regarded as reminders, prompts which trigger stories which are already known, and as verifications, which demonstrate the truth of the tale. Monuments, for Herodotus, preserve and guarantee oral tradition even as they are dependent on it. The tradition, in a sense, explains what a monument means. Without the tradition, monuments mean nothing. At the same time, however, monuments do not lie. As enduring

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[23] Charles W. Hedrick, Jr. (forthcoming). For the other various meanings of this word in Herodotus, see D. Lateiner (1989) *The Historical Method of Herodotus* (*Phoenix* suppl. 23) (Toronto: 1989) 7–9, 50–51).

material traces, real and true (as all real things are), they can preserve and verify the tradition on which they depend. So monuments are not simply frills, inessential addenda to a perfect orality, but necessary, concrete manifestations of the story. The monument is the source and proof of the history of which Herodotus is the author.

This attitude toward words and things is illustrated in a general way by the principles of source criticism which Herodotus adopts. Herodotus distinguishes broadly between two kinds of sources: those which are accessible through sight (ὄψις) and those which are accessible through hearing (ἀκοή).[24] What are heard, in Herodotus, are words. Words, obviously enough, are informative, but they are also notoriously variable and untrustworthy. Different informants may say different things. Worse, words are evanescent: stories will change and vanish with time. What are seen, on the other hand, in Herodotus, are things. Things, unlike words, are concrete, real, and so more trustworthy than words. For Herodotus, as the saying goes, seeing is believing. Unfortunately, things seen, being mute, are also comparatively uninformative. As Herodotus recognizes, seeing is never enough. There must always already be a verbal story, if something seen is to be understood. Accordingly, for Herodotus, sight (ὄψις) is the verification of hearing (ἀκοή). That is, concrete things have a higher priority in the pecking order of truth than words. At the same time, however, the evidence of sight is not self sufficient: it can only be used to supplement the evidence of hearing.

The use of the thing as verification or reminder is dictated by Herodotus' vantage point. When an informant provides him with a story which he does not already know, he is not in a position to speak to its truth or falsity. So he can examine the object for verification. For the informant, however, there is no need to verify the story, as it is already known. The object can, however, usefully serve as a reminder for the informant. Because Herodotus represents himself as the inquirer, the one who is told stories, rather than the ultimate source of stories, objects are far more often conceived as verifications than as reminders.[25]

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[24] For this distinction, see particularly Hartog (1988) 260–309.

[25] For a discussion of objects in Herodotus from a different perspective than I present here, see Carolyn Dewald (forthcoming) 'Reading the World: Significant Objects in Herodotus' Histories', in *Nomodeiktes: Greek Studies in Honor of Martin Ostwald*, eds. R. Rosen and J. Farrell (Ann Arbor: forthcoming).

There are certainly some instances in which things are conceived as reminders, but these are relatively uncommon.[26] Much more often, however, Herodotus uses monuments to prove the truth of a story which he has been told. This use is manifested even in the common ordering and arrangement of ethnographic narratives in the history: first a tale is told, perhaps even a series of tales; the narrative will conclude with a list of the material remains. These objects may in fact have served as the practical 'source' of Herodotus' tale, insofar as they may have prompted the memory of an informant, or of Herodotus himself. In the context of their place in the narrative, however, they serve to prove the story.[27] So the Lydian ethnography (1.26–94) concludes with a tour through the dedications of Croesus, at Delphi and elsewhere (1. 50–51 and 92). The story of Arion's adventure with the pirates and his rescue by the dolphin (1.23–24) ends with a note that 'there is at [cape] Taenarum a small dedicatory offering of Arion, made of bronze and representing a man riding on a dolphin' (1.24). To illustrate Herodotus' practices, one example may stand for many such.

Toward the end of his ethnographic digression on the Scythians, Herodotus turns to the issue of the population of Scythia (4.81). He has heard, he says, 'different accounts of how many they are, some saying they are very many and some few. But they showed me something I could actually see in this matter'. At a place called Exampaeus there was a bronze bowl, big enough to hold 5,400 gallons, and with a thickness of four inches. 'This vessel, the natives say, was made out of arrowheads'. One of the Scythian kings had wanted to know the population of his realm, so he had ordered all his people, on pain of death, to each bring a single arrowhead, which he then had massed into this bowl. 'So much then have I heard about the number of the Scythians', Herodotus concludes.

In this passage, Herodotus distinguishes, as is his habit, between two types of evidence, one oral, the other concrete. The oral accounts which Herodotus has heard are typically many, various and contradictory. Against these he ranges a

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[26] See for instance, 1.92, where Herodotus lists dedications of Croesus of Delphi, and then tells stories about them.

[27] For the common structures of the constituent elements of Herodotus' history, see H. Immerwahr (1966).

piece of evidence which he can actually see, a thing. If we accept the story of the royal census (and even if we do not–the significant issue here is the relationship of the tradition to object, not its truth), then this bowl was used by the Scythians as a general kind of reminder, as a monument showing the results of the collection of arrowheads. Outside of Scythia and the context of their living, popular tradition, the bowl might mean anything or nothing. An archaeological comment, even while emphasizing the likely (though unknown) significance of this monumental vessel, would necessarily fasten first on its use value, which is implied in the mere act of naming it as 'bowl'. For Herodotus, however, the weight and mass of this same bowl can be used as a rough check to verify or refute the various contradictory traditions about the Scythian population which he has been told. He can only know the significance of the bowl through the oral remonstrances of his native informants, which are, in their way, prompted by the bowl. This same object, then, must serve simultaneously as a reminder and as a verification for oral tradition.

The idea of Herodotus' history, like that of any history, ancient or modern, is predicated on preservative and verificatory effects of writing. Even so, Herodotus' predominant attitude toward words, things and writing, as we have seen, is more typical of oral attitudes than of literate attitudes: for the most part he insists on the indissoluble association of meaning with mind. Nevertheless, it might be expected that he would be able to imagine that somehow meaning could be dislodged from human interaction, that it is possible to 'read' originary meaning in things or writing. In a famous passage from his description of Egypt in the second book of the histories, we seem to encounter just such an assertion.

> Thus far I have spoken of Egypt from my own observation, relating what I myself saw (ὄψις) and the ideas that I formed (γνώμη) and the results of my own research (ἱστορίη). What follows rests on the accounts given me by the Egyptians which I shall now repeat (λόγους ἐρέων κατὰ τὰ ἤκουον), supplementing (προσέσται) them with what I myself saw (τί τῆς ἐμῆς ὄψιος) (2.99).

Here Herodotus appears to divide his account of the history of Egypt into two parts. The section following this statement, he says, follows an etiquette regarding sources which is completely consistent with the attitude I have been trying to reconstruct in this article. He will rely on stories told him,

supplementing them with his own, visual observations. These observations will presumably serve as a check on the accuracy and veracity of the stories which he has heard. In the section preceding this statement, however, Herodotus claims to rely only on his observations, not on stories told him. In other words, he is claiming to be able to decipher meaning from things independent of oral report, as one might read writing.

In terms of Herodotus' practices, this claim is preposterous. A passing examination of the early part of Herodotus' second book reveals many passages which are dependent on both report and autopsy.[28] One passage, however, deserves especial notice. As he discusses the geography of Egypt, Herodotus observes that

> Anyone who sees (ἰδόντι) Egypt, without having heard a word about it before (μὴ προακούσαντι), must perceive, if he has only common powers of observation, that . . . it is an acquired country, the gift of the river (2.5).

This passage appears to bear out Herodotus' contention that he has been able to base some of his narrative on simple observation. The relationship between ὄψις and ἀκοή here is more complicated than may first appear, however. The phrase 'gift of the Nile' is itself a quote (which Herodotus does not acknowledge) from an earlier historian, Hecataeus. Herodotus, in other words, is asserting his independence of verbal accounts by means of an unattributed quotation. The first half of the Egyptian λόγος is littered with such unattributed quotations, all posing as Herodotus' independent observations. The extent of his unadmitted reliance on earlier historians has prompted some to suggest that, despite his protestations, Herodotus never visited Egypt at all, and that his narrative here is completely uninformed by any experience with objects, but is based entirely on the verbal accounts of earlier historians.[29]

THUCYDIDES

After Herodotus, the next historian whose works have survived is Thucydides, the historian of the Peloponnesian war. Thucydides is about a generation younger than Herodotus. As

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[28] Again, I have dealt with some of these in my article (forthcoming).

[29] This argument has been made by Detlev Fehling (1989) *Herodotus and his 'Sources': Citation, Invention and Narrative art (ARCA Classical and Medieval Texts, Papers and Monographs* 21), trans. J.G. Howie (Leeds: 1989).

he himself informs us, he began writing his history at the same time as the Peloponnesian war broke out, in 431, and lived to see the end of the war, that is, until sometime after 404 B.C.[30] It is notoriously difficult to pin down Thucydides' thoughts and attitudes about anything, not least because of his practice of reporting speeches of some of his historical actors. As Thucydides himself explicitly observes, his work can be broadly divided into two types of narrative: the account of events and the speeches (1.22). The account of events is cast as a yearly, sequential narrative of the deeds of the war's protagonists. It is told in an impersonal, objective format, though Thucydides occasionally interjects remarks in the first person. 'Quotations', that is, passages of direct and sometimes indirect speech put in the mouths of various leading actors, are scattered throughout the work. Much of what modern readers take to be Thucydides' analysis and reflection about the events of the history is presented in these speeches. Since the speeches are ostensibly delivered by various people, they often represent different, even opposing points of view. So statements may vary from one speaker to another and, of course, from any given speaker to narrator, Thucydides.

The histories of Herodotus and Thucydides seem so different as to be the products of different worlds, not of authors barely a generation apart. These differences might be (indeed have been) charged simply to the characters and idiosyncratic 'geniuses' of the two. It is surely more profitable however, to examine these differences in the context of the changes which occurred in Athenian society at the end of the fifth century. Not the least significant of these changes, for the purposes of this paper, is the increase in the use of writing, both for political or intellectual purposes, in the later fifth and early fourth centuries B.C.[31] As we have seen, Herodotus' attitudes toward history were shaped to some extent by the idea of writing; at the same time, however, certain of his attitudes toward his sources, and even toward his history itself, continued to be dictated by the ideals of a predominantly oral culture. Thucydides' attitudes are much more self consciously, one might even say provocatively, literate in their outlook. As author, he

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[30] For a concise, recent description of Thucydides and his history, see S. Hornblower (1987) *Thucydides* (London: 1987).

[31] See especially Thomas (1989) 15–94; cf. also the passing remarks by William V. Harris (1989) *Ancient Literacy* (Harvard: 1989) 76–77, 80–81.

writes, not speaks; his history itself he calls a συγγραφή, very literally 'a written composition'.

In a famous passage, Thucydides criticizes the oral compositions of Herodotus and other earlier historians, remarking that his own history

> is composed (ξυγκεῖται) as a possession forever (κτῆμα ἐς ἀεί), rather than as an entry in a contest, to be heard only at the present instant (ἀγώνισμα ἐς τὸ παραχρῆμα ἀκούειν) (1.22).

Here Thucydides is distinguishing his (written) composition from earlier oral performances on the basis of its permanence. He expects that his history will survive him and his own momentary oral representations and elaborations and speak in the absence of its author to future generations. To express the endurance and autonomy of the history, he links writing to material culture, to the object (κτῆμα).[32] The analogy suggests not only that writing is like a monument (a commonplace in Greco-Roman antiquity), but also that monuments are like writing. Thucydides conceives of things as a kind of writing, which can yield up their originary meanings to those who examine them independent of any oral explanation. Their meaning, in other words, appears to persist in the absence of those who created them, even of those who know, much as the meaning of writing does. This 'writerly' conception of things, the notion that things can convey autonomous and originary meaning, is not simply a central difference between Herodotus' and Thucydides' interpretations of the human past; it is the precondition, the essential theoretical presupposition which guides modern archaeology.

The practices of Thucydides' history bear out this programmatic statement. Thucydides is quite prepared to imagine that things have a meaning which subsists independent of memory, a meaning which is accessible to the historian with insight. Consequently he is able to exploit objects in a manner comparable to that of the modern archaeologist: Thucydides uses things as a witness for the past, a witness which is independent of and can even contradict or disprove explanatory verbal accounts. The meaning of things, however, is still more complicated for Thucydides: if things are more than mere indisputable, existential truths which affirm what is known by

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[32] For this passage, see again my article (forthcoming).

their very being, if they can communicate symbolically, refer, as do words, then surely they can lie as well.

Utility and Meaning

In order to interpret the meaning of objects (and for that matter, words or anything else), it is necessary to have some pre-existing theory, a general framework against which the specific object can be measured. A grammar, syntax and vocabulary are required. In this sense, Herodotus' attitude is accurate: to read or understand anything, be it writing or material culture, one must already somehow know. Specific meaning only gives itself up to a more general understanding. But, as advocates of the 'hermeneutic circle' would emphasize, it is insufficient to simply assert the priority of this 'general understanding'. Granted the specific cannot be known without a general vision, but conversely, a general vision cannot be generated without specific knowledge.[33] There is no correct place to begin in this circle of meaning; it is necessary and sufficient only to begin.

Thucydides bases his assertions of the autonomous meaning of things on a reasoned argument: material meaning, he claims, can be deciphered in terms of a dichotomy of use versus boasting. Baldly stated, this dichotomy perhaps sounds suspiciously modern, too close to the logic of traditional archaeology, or the old rallying cry of socialism: 'production for use, not ostentation'. I cannot here attempt to provide a detailed account and critique of the 'utility versus ostentation' dichotomy and its role in modern and ancient interpretations of material culture, though it is obviously relevant to this discussion. I will, however, try to outline in a general way Thucydides' conception of the opposition and its role in his practices. I would obviously not claim that there are no significant differences between Thucydides and a modern materialist, say Marx or Childe. Nevertheless, because of his willingness to reason out and apply a 'grammar' of material meaning, Thucydides is practicing in his history something recognizable as 'archaeology', in the modern sense of the term.[34]

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[33] For the 'hermeneutic circle' see e.g. David Hoy (1978) *The Critical Circle: Literature, History and Philosophical Hermeneutics* (Berkeley: 1978).

[34] I do not mean to imply that other ancient authors do not also attempt to interpret objects according to a criterion of utility. See most notoriously Aristotle, *34.Politics* I, 1256b 26–1257b 39.

A statement of the discrimination between use and ostentation is provided in one of the most famous speeches in Thucydides' history, the funeral oration. The speaker, Perikles, praises the character of the Athenian state. The city of Athens is marvelous and admirable for many reasons, he says: in particular the Athenians manage to be connoisseurs (φιλοκαλοῦμεν with economy (μετ' εὐτελείας) and intellectuals (φιλοσοφοῦμεν) without effeminacy (ἄνευ μαλακίας). Wealth (πλοῦτος) they use (χρώμεθα) more as an occasion for action (καιρῷ ἔργου) than for exaggeration of speech (λόγου κόμπῳ), and do not consider it shameful to confess (ὁμολογεῖν) poverty, but take it as very shameful not to take action to avoid it (διαφεύγειν ἐργῷ) (2.40).[35]

Thucydides has Perikles claim here that the Athenians generally believe that the value of wealth lies in its use for action, in what it can effect, rather than in its application for pointless, impotent display. A hierarchy of objects is implied here, which is based not on qualitative differences among specific concrete things, but on abstract categories which in the modern world would be called use and ostentation. It is perhaps too easy for a modern critic (such as myself) to slip from Thucydides' text to this contemporary cliché; it should be kept in mind that Thucydides expresses this contrast in terms of an equally common ancient Greek dichotomy, that is between word (λόγος) and deed (ἔργον).[36] This is also the way in which Herodotus would express the opposition between verbal account and monument, between the referential but ambiguous and the true but final. Thucydides here is taking this opposing qualities of word and thing and applying them to the thing alone; that is, he is effectively claiming that the thing can be used for either deed or word. He is asserting, in other words, the cohabitation of two functions in objects, one of which is final and true - what I am describing as action or use, ἔργον; the other of which is communicative and deceptive - what I am describing as word or ostentation, λόγος.

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[35] For general discussion and bibliography on this complicated passage, see J. Rusten (1985) 'Two lives or three? Pericles on the Athenian character (Thucydides 2.40.1–2)', *Classical Quarterly* 35 (1985) 14–19. For the complex Athenian attitudes toward wealth, see J. Ober (1989) *Mass and Elite in Democratic Athens: Rhetoric Ideology and Power of the People* (Princeton: 1989); L. Kurke (1992) 'The politics of (ἁβροσύνη) in Archaic Greece', *Classical Antiquity* 11 (1992), 91–120.

[36] For this contrast in Thucydides and in Greek generally see Adam Parry (1981) *Logos and Ergon in Thucydides* (New York: 1981).

Word and deed are obviously complicit with one another insofar as they share the same object: the deed is the factual value of the object in its potential for use. The word on the other hand might be described as a representation which, like any representation, may be sincere, or deceitful or exaggerated. But a representation of what? As will appear, for Thucydides this representation is precisely a representation of the thing's potential for use. So, to elaborate on Perikles' statement, it is always possible for one and the same thing (in this case, wealth) to be used in either way, that is either as an occasion for action or as the sign of that same action: the Athenians prefer the former. It would be a mistake to imagine that there are always two discrete and mutually exclusive meanings for any given object at any given moment. It would be more accurate to maintain that objects are always both simultaneously: a potential use and a sign of that potential use.

A further clarification of the contrast between use and ostentation is provided by the same speaker, Perikles, in another speech later in the second book. It is early in the Peloponnesian war. The Spartans have invaded Attica for a second time and ravaged the land. The Athenians, confined to their city and infested by the plague, are beginning to contemplate making a peace with Sparta. Perikles urges them to remain steadfast: even though their farms have been destroyed, he says, they are yet in possession of their fleet. In fact, Perikles argues, the fleet is far more to be valued than the farms: The sort of power (δύναμις) which the fleet provides does not appear (οὐ φαίνεται) in the use (κατὰ τὴν χρείαν) of their lands and houses, even though the Athenians may consider the loss of these as a great deprivation. They should not worry about such things, however, but should consider them of as little worth as gardens and other ornaments of wealth (ἐγκαλλώπισμα πλούτου) as compared to the power of the fleet (2.62.3).

This passage lays out clearly the essential issues and stakes involved in the interpretation of objects, as Thucydides understands them. To begin with, the conflicting functions of material meaning are here explicitly described as use and ostentation. The relative importance of each is unambiguous: the power manifested by the fleet is something completely different than that of the use of farms and houses; in contrast, the land possessions of the Athenians should be regarded as trifling trinkets, of little importance. An ultimate referent of these functions is also suggested: power. The comment about

the relationship between power and use is particularly important: power appears in the uses of things. Use in other words, is a manifestation of power, a kind of symbol. In this case, the Athenians have misunderstood, or more specifically, overestimated the power which the utility of their farms indicates. With respect to the power to which their use gives access, they are so far outstripped by the fleet that they should be regarded as useless; as mere ornaments which accompany and indicate wealth, but are not wealth itself (as useful objects are).

Power, then, is the standard by which Thucydides evaluates things. The utility of the object, the thing conceived as a means by which to make things happen, is for him an absolute reflection of power. The choice of utility as a criterion for evaluating things is no accident. It is arguable, in fact, that use value is universally the most heavily and thoroughly socialized meaning of a thing.[37] The extent of this socialization is clearly demonstrated in the standardization of useful objects, for example, tools. Standardization, that is formal repetition or reproduction, is one of the hallmarks of a sign. To be commonly recognizable, the sign must be replicable.[38] So, to the extent that the sign is replicated, its meaning is socialized. For example, if the sound 'cat' cannot be repeated, it cannot serve as a word. In the sphere of material culture, we might say that many things can be used as hammers: in a pinch one can improvise with a rock or shoe. The socially recognized meaning of 'hammer', however, is reserved for a particular object, which is known as a hammer by its standardized features.[39]

An important feature of useful objects is that in them meaning often appears to be entirely subsumed in their function. In Aristotelian terms, the meaning of a tool is determined and guaranteed by its τέλος; that is, a tool does not simply refer to its end, it is a predetermined medium, a conduit to that end. Conceived as a function, it is linked to its end as 'naturally' as, say, fire is linked to smoke. In fact, in the tool, meaning

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[37] For a short and excellent summary of this point see R. Barthes, 'The semantics of the object', in Barthes (1988) 179–90; B. Olson (1990) 'Roland Barthes: from Sign to Text', in *Reading Material Culture*, ed. C. Tilley (Oxford: 1990) 163–205. For the full statement of the position, see R. Barthes (1985) *The Fashion System*, trans. M. Ward and R. Howard (London: 1985).

[38] Derrida has good comments on this aspect of the sign in his discussion of citation: (1983) *Margins of Philosophy* (London: 1983).

[39] Compare with this point Levi-Strauss' comments on bricolage and science versus art: (1966) *The Savage Mind* (Chicago: 1966) 16–33.

and function are often conceived as being at odds. The appearance of an excellent tool is regarded as irrelevant; or better, as entirely dictated by the efficiency of the tool's function, and hence as meaningless. Appearance, and particularly appearance which cannot be explained as in some way governed by efficiency and function, is castigated as ostentation, considered useless and hence to be suppressed.[40] One does not ask of a tool how it looks, but what it is for.

A tool, then, is not normally defined in terms of its formal appearance as a meaningful thing, but as a thing for something else. A hammer, for instance, might be described either formally as an object with a long handle, heavy head (reminiscent of a clenched fist) and claw (reminiscent of rigid, outstretched fingers), or differently (and more abstractly) as a device for driving nails. The perfectly useful thing would vanish as an object of description: entirely transitive, it would be a mediation whose meaning and appearance is utterly suppressed and absorbed in its end. Useful things in a sense refer to some external imperative. So, in the case of archaeology and materialist history, tools are often regarded as a means of human adaptation to the environment. Such tools by their function and efficiency, bespeak the needs of people, the exigencies of the place, the advance of technology: all natural finalities. Useful objects, then, are conventionally seen as instruments, not as conveyors of meaning.

In Thucydides, the general logic by which useful things are given significance is the same as in modern materialist archaeologists and historians: tools are instruments, which do not so much have a meaningful appearance as they mediate an ultimate effect. For Thucydides, however, the end of the tool is not environmental adaptation or the like: for him, useful things provide a means and access to power. So, in the passage just discussed (2.62) Thucydides has Perikles provide an explicit statement of the connection between useful things and power. He differentiates between the Athenian possessions on land (mere 'ornaments', he calls them) and the power of the fleet, something widely different. What is at stake in the choice between ships and ornaments is power. Material resources, when used, will effect results; the ability to act, to make things happen is power. In the case of the fleet, use leads to political domination. Because of this link between use and power, be-

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[40] Barthes (1985) is particularly good on this tension.

cause useful objects are a means to power, it will be possible for the historian to extrapolate political power, taking useful material objects as its index.

Thucydides puts this conception into historical practice on more than one occasion. A striking instance is provided by his treatment of the Trojan war (1.9–11).[41] In his review of early Greek history, Thucydides argues, against contemporary prejudices, that the power of the Greek armament against Troy 'surpassed all before it, just as it fell short of modern efforts.' He proves his contention by invoking the testimony of the Homeric poems. He takes the witness of these poems literally, refusing to allow even for 'the exaggeration which a poet would feel himself licensed to employ'. He then proceeds, following Homer to enumerate painstakingly and to deprecate the numbers of ships, oarsman, and marines who went on the expedition, as well as the limits of the armada's accomplishments. He concludes that because of lack of money (ἀχρηματίαν) earlier expeditions were weak, and the Trojan expedition, though more famous than any of its predecessors, is shown on the evidence of its deeds (τοῖς ἔργοις) to be inferior to its reputation (φήμης) and to the current opinion of it formed by the epic poets. The expedition to Troy, judged by the standard of all useful things, that is by its deeds, by what it accomplished, proves wanting. So Thucydides is able to 'read' the meaning of the Homeric count of ships in the light of the grammar of utility, and demonstrate his larger point: not even the power of the ancient Trojan armada is adequate to contemporary standards.

Despite Thucydides' advocacy of use as a standard of interpretation for objects, the meaning of things remains for him deeply problematic. Things are ambiguous for Thucydides because he never forgets his role as an interpreter of objects. Things may indeed have use values, and conceived as use values, they are nothing in themselves. Their relationship to a final meaning, power, is purely mechanical. Nevertheless, despite attempts to suppress meaning in the name of function, even tools, insofar as they are seen and understood by people, con-

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[41] There is a long history of commentary on this passage, ably summarized in S. Hornblower (1991) *A Commentary on Thucydides* I, (Oxford: 1991) 31–37. Most scholars have been quite critical of his practices: see e.g. M.I. Finley 'Myth, Memory and History', in Finley (1975) 11–33 and especially 18–20.

vey meaning. Obviously enough, the range of connotation for things (as for words) is broad and may be very subjective. In the case of standardized, useful objects, such as tools, however, it can be said that at a minimum they stand as signs of their functions. So, for Thucydides, wealth can be used to give access to power. At the same time, however, wealth is the sign of that access to power and, conceived as a sign, its meaning is problematic.

Umberto Eco once wrote that a sign is anything that can be used to lie.[42] Tools, conceived strictly as use values, cannot lie: for one using them, they give the potential for action, access to effects - providing always that someone uses them. For one 'reading' them, however, they are not simply predetermined functions, but the signs of effects, manifestations, and insofar as they indicate their ends, their representations may be either true or false. To the historian or archaeologist (unlike the improvisational artisan) useful things are as ambiguous and problematic as any other sign.

In Thucydides' history, things are not only understood as signs by the historian, but also by the actors in the history. In particular, things-as-signs are subject to the corrupting influence of boasting, bragging, exaggeration, or in modern terms, ostentation. Useful things are not necessarily applied efficaciously to provide power; some are used ostentatiously to refer to power. Any useful thing which is 'read' is no longer a tool, but a sign. In the end, for Thucydides, the discrimination between use and ostentation is indeterminable.

The problems involved in differentiating use and ostentation emerge notably in the sixth book of the history. Here Thucydides recounts the events leading up to the Sicilian expedition, the great armada which the Athenian sent off to conquer Syracuse and the rest of that island. As the Athenians pondered the wisdom of this expedition, two Athenian statesmen debated the issue: Nikias, who opposed the expedition, and Alcibiades, who was the moving force behind it. Nikias criticized Alcibiades for his youth, inexperience and wasteful lifestyle: the entire expedition, he suggested, was cooked up by Alcibiades for his own private profit: because he had lost heavily on his stud of race horses, he wanted to recoup his money by a military expedition (6.12). Thucydides evidently agrees with Nikias' criticisms of Alcibiades. He comments himself,

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[42] U. Eco (1979) *A Theory of Semiotics* (Indiana: 1979) 7.

in a rare authorial intrusion into the narrative, that because of Alcibiades' preeminence in Athens, he had given in to his desires beyond what his wealth would bear. This behaviour, says Thucydides, contributed later in no small way to the destruction of the Athenian state (6.15).

Thucydides' condemnation of Alcibiades is as unambiguous as any statement ever is in the history. Nevertheless, immediately following, the historian puts an answer into the mouth of the young reprobate, an answer which raises disturbing questions about the method of interpretation of the material culture of the historian himself: Alcibiades argues that ostentation is useful too. He claims that the behaviour for which he is notorious has brought glory to himself and his ancestors, and advantage (ὠφελίαν) to the country. The rest of the Greeks assumed that Athens was great beyond its real power (ὑπὲρ δύναμιν) because of the splendour (διαπρεπεῖ) with which he and his maligned stud of horses had represented the state at the Olympic festival, though before these same Greeks had expected to see Athens destroyed by war. 'By custom such splendour is honorable, and through the performance (ἐκ τοῦ δρωμένου) power also is at the same time implied (καὶ δύναμις ἅμα ὑπονοεῖται)'. Furthermore, says Alcibiades, if he has shone (λαμπρύνομαι) at Athens by providing choruses or in any other way, it is natural that his fellow citizens should envy him; this too, however, appears to foreigners to be strength (ἰσχύς). And this folly (ἄνοια) is not useless (ἄχρηστος), if someone chooses, at his own private expense, to benefit not only himself but also the city (6.16). Tools, then, for Thucydides (or Alcibiades) are simply functional values. When read, however, they can be used to lie. The display of things, says Alcibiades, is useful too, not because of any real power which it generates, but because of the impression of power which it fosters. This impression, though it may be false, is nonetheless advantageous.

Alcibiades not only asserts that ostentation creates an impression of power in the contemporary world, but also among posterity: such as have excelled in splendour (λαμπρότητι) of anything at all will be envied during their lifetimes, particularly by their equals, but also by those with whom they are familiar. Among future generations (τῶν ἔπειτα ἀνθρώπων), however, they will be claimed as kindred by some, though there is no connection, and, whatever their country may be, it will boast of them, not as of strangers or failures, but as of fellow citizens and benefactors (6.16). These comments amount to a

direct response to the historian who has just been so critical of the role of Alcibiades in the demise of Athenian power. Those who are concerned with knowing the past, historians like Thucydides, or archaeologists, will accordingly encounter such exaggerations and ostentatious falsehoods, and must somehow know to distinguish them from the reliable indications of true power. Thucydides, as he says, disapproves of Alcibiades' attitude, even considers it a danger to the city. Nevertheless, he is aware that Alcibiades speaks the truth: even the most useful things can be used to lie. These lies may themselves be useful, in that they produce an intimidating impression of power. Such displays may mislead not only contemporaries of Alcibiades, but the historian as well. After all, as Alcibiades says, posterity thinks well of individuals who make such displays.

Early in the first book, Thucydides discusses this problem as it applies to his own history and method. Impressive material remains no more indicate great power than the absence of such remains indicates its lack. When one examines Mycenae, and other towns of the age of the Trojan war, these places appear by contemporary standards insignificant.[43] The passage is remarkable and merits full quotation:

> No one who examines the indications precisely (ἀκριβεῖ σημείῳ χρώμενος) would for that reason mistrust the estimate of the size of the expedition against Troy provided by the poets and story (λόγος). For if the city of the Lacedaimonians were to be deserted, and only the sanctuaries and the foundations of the buildings were to be left, I imagine that, after a long period of time, some future generation (τοῖς ἔπειτα) would have great mistrust of their power (δυνάμεως) in relation to their reputation (κλέος). And yet they occupy two of the five parts of the Peloponnesos and lead the whole, as well as numerous allies outside. Nevertheless, since the state has not been consolidated or outfitted with expensive shrines and buildings, but is settled in scattered villages in the ancient manner of Greece, it might appear inadequate (φαίνοιτ' ἂν ὑποδεεστέρα). However, if Athens were to suffer the same misfortune, I suppose that one would infer from the visible appearance (φανερᾶς ὄψεως) of the city that its power is double what it is. Therefore it is not reasonable to be mistrustful or to look at the appearances (ὄψεις) of cities rather than at their powers (δυνάμεις); but to believe that that expedition was the greatest of those before it, but fell short of those now, if it is necessary in this to trust the epic of Homer, which, being a poet, he is likely to have exaggerated (1.10).

[43] Hornblower (1991) 33–35, has some worthwhile comments on this passage.

The problem with using material culture as an index of power is that, like words, they can be misleading. Sparta, with its scant material remains, has greater power than opulent Athens. This reality is contrary to all impression which the two cities make. Therefore the wise historian or archaeologist will look to power rather than appearance.

Wood and Walls: Ships and Fortifications in Thucydides

Having said all of this, it may be well to look at a specific interpretation of material culture in Thucydides' history. Thucydides' history is not so replete with objects as is that of Herodotus. Nevertheless, what he does choose to include and its analysis is astonishing, almost modern in its attitude.[44] Here I will focus on two kinds of object, which have received little attention from scholars (at least as evidence for Thucydides method and practice of history): ships and walls.[45] Both, for Thucydides, can be conceived as useful, and so can serve as indices of the power of their possessor. Both can also be read as signs, and so used to mislead. Despite the limited attention they have attracted from modern scholars, it would be difficult to overestimate their importance in the history. It would not, in fact, be excessive to claim that the entire history is structured around their interpretation.

Thucydides' emphasis on walls and ships is characteristically

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[44] Most modern readers have deprecated Thucydides' interpretation of material culture. See e.g. R.M. Cook (1955) 'Thucydides as Archaeologist', *Annual of the British School at Athens* 50 (1955) 266–70; M.I. Finley, 'Myth, Memory and History,' in Finley (1975) 11–33 and especially 18–20. For an interpretation of Thucydides' attitude toward monuments as neglect prompted in part by Thucydides' disdain for Herodotus' practices, see Hornblower (1987) 30–33, 91–92.

[45] The importance of this pair has obviously been noted by some. For walls in the writing of Greek history, see Y. Garlan (1968) 'Fortifications et histoire grecque', in *Problèmes de la guerre en Grèce ancienne*, ed. J.-P. Vernant (Paris: 1968) 245–60, and esp. 255–58. The same author deals with the same issue again in his (1974) *Recherches de poliorcétuque grècque (Bibliothèque des écoles françaises d'Athènes et de Rome*, fasc.223) (Athens: 1974). 1.103.1', *Historia* 19 (1970) 313–25, who notes briefly on page 312 that in Thucydides, walls are 'the ultimate symbol of power.' For naval power, see particularly the famous article by Arnaldo Momigliano, reprinted in his (1960) *Secondo contributo alla storia degli studi classici* (Rome: 1960) 57–67: 'Sea Power in Greek Thought'. Of lesser interest is his essay 'Terra marique,' reprinted in the same volume, 431–446. The most relevant and explicit ancient description of the relationship between naval armaments and political power is the 'Athenian constitution' of the so-called 'old oligarch' (often printed with collections of Xenophon's works).

insightful. This emphasis is probably not his own discovery, however. There are many other indications that other Athenians in the late fifth century realized not only the utilitarian value of the ships and walls of the Athenian empire, but their symbolic value as well. Already, at the time of the Persian wars, a link between ships and walls has been established: one thinks of the notorious 'wooden walls' oracle in Herodotus (6.141–143).[46] By the late fifth and early fourth centuries B.C., the correlation of ships and walls with political power was doubtless a common topos in Athens.

The most interesting evidence for the connection between ships and walls and power (outside of Thucydides) comes from a speech by the orator Andocides, entitled 'On the Peace with Sparta'. This speech was probably delivered in 391 B.C., more than ten years after the defeat of Athens in the Peloponnesian war. As a result of their defeat by Sparta, the Athenians had had to surrender their walls and fleet. Conflict between Athens and Sparta quickly recommenced. In 391 B.C. a peace was negotiated between the two states. By the terms of this peace, Sparta recognized Athens' claim to her fleet and fortifications. These concessions should be sufficient, says Andocides, because it is from walls and ships that their empire originally sprang:

> there was once a time, gentlemen, when we had no walls or fleet. It was when we acquired them that our prosperity began. If you have a similar desire for prosperity today, then make sure of your walls and ships' (Andocides 3.37).

More than prosperity, the Athenian empire sprang directly from the possession of ships and walls. In a passage notorious for its chronological confusions,[47] Andocides sketches out the sources of the growth of Athenian power in the mid fifth century, describing that growth with particular emphasis on walls and ships, both those which the Athenians got for themselves and those of which they deprived others (Andocides 3.3–16).

These walls and ships of the Athenians led directly to the Peloponnesian war, which Athens lost to the Spartans in 404

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[46] For this oracle, and its equation of ships and walls, see A. Bauer (1884) *Themistokles: Literary, Epigraphical and Archaeological Testimonia* (Leipzig: 1884) (re-edited with supplements by F. Frost) (Chicago: 1967), 36–38, where are collected a host of literary parallels.

[47] See W.E. Thompson (1967) 'Andocides and Hellanicus', *Transactions of the American Philological Association* 98 (1967) 483–90, who is concerned with literary sources for Andocides' narrative; compare this with R. Thomas' appeal to a 'family tradition in Thomas (1989) 136–44.

B.C. As part of the settlement, the Spartans destroyed the Athenian navy and walls. As Andocides notes,

> Then came defeat. And not only did we lose our empire, our walls and our fleet were also seized as securities by Sparta. The fleet she confiscated and the walls she demolished, to prevent our using them as the foundations of a fresh Athenian dominion (Andocides 3.39)

Andocides' discussion of ships and walls is clearly based on a criterion of utility: ships and walls are good for attaining and maintaining power; they produce empire. This conclusion is based on the use values of the two types of objects: walls are good for defence, ships are good for domination. At the same time, he is concerned with (though less explicit about) the symbolic value which this utility produces. If ships and walls produce empire, then they must also be the signs of empire. The functional link produces a symbolic value.

Andocides' account is in no way comparable to Thucydides' careful analysis. In his history, of course, Thucydides is aware of the use values of these objects - it is from their use values that his interpretation of them springs. He is, however, able to reverse the process which we see in Andocides and consider them not only as instruments of power, but also as evidence of power, and hence, as ambiguous. He moves them from their functional value to that of signs.

As the common English title suggests, the focus of Thucydides history is the Peloponnesian war, which began in 431 B.C. and ended in 404 B.C. As it has been preserved, the history is incomplete: it breaks off inexplicably, in the middle of a sentence, in the twenty first year of the war (411 B.C.). Nevertheless, it clearly was Thucydides' intention to bring the history down to the end of the war in 404 B.C.[48] As he remarks, he lived through the twenty seven years of the war himself, being at its outset of sufficient maturity to comprehend its events (5.26).

The account of the war begins with the second book of the history. The first book is devoted to a series of 'digressions', designed to provide the background to the outbreak of the

[48] For a review of the notorious problem of the incompleteness of Thucydides' history, see e.g. A.W. Gomme, A. Andrewes and K.J. Dover (1981) *A Historical Commentary on Thucydides*, vol. 5 (Oxford: 1981) 361–444; more briefly, see chapter 6 in Hornblower (1987) 136–54. A stimulating account of the overall structure of the work, including the missing ending, are to be found in H. Rawlings (1981) *The Structure of Thucydides' History* (Princeton: 1981).

war.[49] The first of these, the so called 'archaeology', is an account of the rise of states in Greece in earliest times (1.1–22).[50] Next, after a lengthy account of the immediate and remote causes of the war (1.23–88), Thucydides turns to the rise of Athenian power in a section conventionally called the 'pentekontaetia', the 'fifty years' (1.89–117), named for the fifty year interval between the Persian wars (490–480 B.C.) and the outbreak of the Peloponnesian war (431 B.C.).The book concludes with an account of the events which lead immediately to hostilities (1.118–146). In all of this, a connecting theme is power and its material evidence: walls and ships.

In the archaeology, Thucydides is concerned to explain, among other things, the origins and development of the power of states in early history. The story is largely one of the acquisition of walls and ships. So, for example, Thucydides remarks that in ancient times there was no settled population in Greece.[52] Life was precarious,

> without commerce, without freedom of communication either by land or sea, cultivating no more of their territory than the exigencies of life required, destitute of capital, never planting their land (for they could not tell when invaders might come and take it all away, and if invaders did come they had no walls to stop them (1.2).

Thucydides then proceeds to give his account of the origins and growth of naval power (1.3–15).[53] It is less often emphasized that he is also concerned in this section to give an account

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[49] One of the most famous (and best) essays on the organization of Book I of Thucydides is to be found in J.H. Finley, Jr. (1967) 'The Unity of Thucydides' History', in his *Three Essays on Thucydides* (Cambridge MA: 1967). With this compare N.G.L. Hammond (1952) 'The Arrangement of Thought in the Proem and in other Parts of Thucydides I', *Classical Quarterly* 2 (1952) 127–41.

[50] For discussion and bibliography, see J.R. Ellis (1991) 'The structure and argument of Thucydides' archaeology', 10 *California Studies in Classical Antiquity* (1991), 344–75. Cf. further V. Hunter (1982) 'Mankind's Progress to Civilization in Greece: Thucydides' Archaeology and Problems of Power', in her *Past and Process in Herodotus and Thucydides* (Princeton: 1982) 17–49.

[51] On this section, see for example the article by R.A. McNeal (1970) 'Historical Methods and Thucydides 1.103.1', *Historia* 19 (1970) 313–25.

[52] Generally, on Thucydides' account of 'prehistory' and the beginnings of civilization, see M.H. Marshall (1975) 'Urban Settlement in the Second Chapter of Thucydides', *Classical Quarterly* 25 (1975) 26–40.

[53] Thucydides' treatment of ancient Crete in this section has surely drawn the most attention: see especially R. Hägg and N. Marinatos (1984) *The Minoan Thalassocracy: Myth and Reality* (*Skrifter Utgivna av Svenska Institutet i Athen* 4, 32) (Stockholm: 1984).

of the beginnings of fortification. The first person, he says, who, according to tradition, established a navy, was Minos, king of Crete. Minos used this navy to make himself master of the Aegean (1.4). Even before Minos, certain Greeks and Barbarians had used ships for piracy, falling upon 'towns unprotected by walls and consisting of a mere collection of villages' and plundering them (1.5). It is precisely the threat of ships which prompts the use of walls: as navigation and trade become more of a common practice, the shores became the sites of walled towns, and the isthmuses were occupied for reasons of commerce and defence. 'But the old towns (presumably unwalled), on account of piracy, were for the most part built away from the sea' (1.7).[54]

The process of fortification increased further after the time of Minos. By using his navy, Minos had managed to colonize the islands and expell the pirates. Paradoxically, when the pirates were gone, the building of fortifications increased. 'The coastal folk, applying themselves more seriously to the acquisition of wealth, became more securely settled than before, and certain even fortified themselves with walls, as they became ever richer'. These walls are not instruments of defence, or emblems of insecurity: the external imperatives for defence at this time have diminished. They should instead be understood in terms of the increasing power and confidence of the towns they envelop. Thucydides goes on to remark, by way of explanation: 'Because of their addiction to gain (ἐφιέμενοι τῶν κερδῶν) the weak were reconciled to their enslavement to the stronger, and, since they had possession of wealth, the more powerful could reduce the smaller towns to subjection (1.8).[55]

Thucydides then proceeds to his famous assessment of the power underlying the expedition against Troy (1.9–11). His judgement is based on a simple calculation, which, as he says, does not even make allowance for poetical exaggeration, of the numbers of ships and men who went to Troy. In this passage, there is also a noteworthy extrapolation based on the fortifications which the Greek force supposedly built on the

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[54] For this process, see F. Winter (1971) *Greek Fortifications* (London: 1971) 8–12.

[55] A. W. Gomme (1945) *A Historical Commentary on Thucydides*, vol. I (Oxford: 1945) 108, glosses this sentence: 'in times of economic expansion the non-possessing classes are content to go on being non-possessors, because their standard of living is rising, while the possessors of capital increase both their possessions and their power'.

Trojan plain: the Greeks must have obtained a victory when they first arrived on the plain before Troy; without such a victory, they could never have built the fortifications of the naval camp (1.11).

After the Trojan war the power of Greece falls off until the era of great tyrannies. The rest of the Archaeology is devoted to an account of the development of naval resources down to the end of the Persian wars, when Athens and Sparta finally confronted one another.

As Thucydides and many others since have pointed out, the beginnings of hostilities between Sparta and Athens are problematic. Certainly the two states had been at odds for sometime before the outbreak of the great war. Thucydides outlines the various causes and complaints that each side alleged, but notes that in his opinion the truest cause was the one which was least apparent: 'the growth of Athenian power, and the fear in Sparta which this inspired, made war inevitable' (1.23). Thucydides is very specific about the instrumental source and outward sign of this Athenian power which the Spartans so feared: it is the acquisition of walls and ships.

Thucydides' purpose in recounting the events of the pentekontaetia is to explain how Athens got and fostered power. In the first sentence of the section, he announces this theme: 'the way in which Athens came to be placed in the circumstances in which her power grew was this' (1.89). He then proceeds to tell, in great detail, how the Athenians managed to rebuild their walls, in spite of Spartan opposition, after the Persian wars (1.89–93). The Spartans desired no one to have walls. They urged the Athenians not to build new fortifications, but rather to help them to tear down any other walls still standing outside of the area which they dominated, the Peloponnesos: this territory, the Spartans argued, would suffice as a base for retreat and offence for all Greece. The Athenians ignored the Spartans' wishes, and continued to reconstruct their walls. Thucydides lavishes elaborate attention on the construction of the walls and their contemporary appearance. Since the Spartans opposed the work, the utmost speed in their construction was necessary. In their building, the Athenians used any stones which lay to hand.[56] 'To this day', Thucydides remarks,

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[56] See R.E. Wycherley (1978) *The Stones of Athens* (Princeton: 1978) 11; cf. further, R. Garland (1985) *The Greek Way of Death* (London: 1985) 4 and 122.

> it is clear that the wall was built in a hurry. The foundations are laid of stones of all kinds, and in some places not well fitted, but set in whatever order anyone brought them. Many markers from tombs and worked stones were put in (1.93).

Thucydides next proceeds to tell how the Athenians succeeded the Spartans as the leaders of the allied Greek fleet (1.94–97). This fleet had been collected to fight the Persian invaders, and was originally under the command of Sparta. A Spartan general, Pausanias, however, so abused his position and alienated the allies, that they converted their allegiance to the Athenians. The Athenians rapidly converted the alliance against the Persians to a 'Delian league', with themselves at the head. 'After the Athenians took over the leadership in this way, with the concurrence of the allies because of their hatred of Pausanias, they determined which cities (of the alliance) should contribute money against the barbarian and which ships' (1.96).

According to Thucydides, then, the origins of Athenian power can be traced to two specific events: the rebuilding of the walls of Athens, and the foundation of the Delian league, supported by the allied fleet. Both of these developments were accomplished in the face of Spartan opposition, or at the immediate expense of Sparta, giving further grounds to 'Spartan fears of growing Athenian power'.

Thucydides next proceeds to give an account of the period between the Persian wars and the outbreak of the Peloponnesian war, noting that 'the history of these events contains an explanation of the growth of the Athenian empire' (1.97). The growth of Athenian empire, for Thucydides, is also a matter of ships and walls.

To begin with, the Athenians had the most powerful fleet, so that it was easy for them to reduce any allies who tried to leave the alliance. The allies had a large measure of responsibility for this, since many of them preferred to make contributions to the league in money rather than in ships. So Athens was able to increase her own fleet at the expense of the allies, and those who revolted lacked 'the resources and experience for war' (1.99). Thucydides remarks that the Athenians systematically deprived their allies of their fleets, imposing instead contributions of money (1.19). He does not mention here (though he does in specific cases) that it was also a common policy to deprive the allies of their walls. When a city revolted or was forcibly compelled to join the league, the most

common settlement imposed by the Athenians was destruction of the walls and confiscation of the fleet. Once stripped of walls and fleet, tribute was commonly taken from such states in money. So, for example, when the Athenians conquer Thasos, they require them to tear down their walls and give up their ships (1.101); when they defeat the Aiginetans they impose a comparable settlement (1.108). This pattern is repeated many times in the course of the development of the Athenian empire: Samos (1.117), Potidaia (1.56–7, 62–64), and Lesbos (3.2–3, 50), all are deprived of ships and walls by the Athenians. On the mere suspicion of revolt, the Athenians force the Chians to raze their walls (but do not confiscate their fleet) (4.51).

In territories which the Athenians inhabit or occupy they build walls. They take over Megara, and build long walls to the Sea (1.103). In Athens, as well, they build long walls to join the city with the harbor (1.107). The building of the long walls is explicitly linked to the growth of power by the Corinthians, who complain to Sparta of their complicity in the growth of Athenian power: 'for all this you are responsible. You are the ones who at first allowed them to fortify (κρατῦναι) their city after the Persian war, and later to erect the long walls' (1.69).

This is a mere sampling of some passages which illustrate in a relatively unambiguous way the link between power and the tools which effect it, that is ships and walls, in Thucydides' history. This list could be multiplied to a pointless length. The history is full of detailed accounts of ships, their strengths and numbers and possessors; of walls, how they are built, where they are built. This information is often provided in unparallelled length and specificity.

Perhaps the best proof of the importance of ships and walls in Thucydides comes not from an assertion, but from a denial. The expedition which the Athenians sent against Sicily and Syracuse in 415, as everyone knows, was a disaster. The Athenian attempt to encircle the walls of Syracuse with their own wall, so elaborately described by Thucydides (6.99–101, 7.2–11), was a failure. The Athenian fleet was then trapped inside the harbor of Syracuse and utterly destroyed (7.59–72). So the Athenian army found themselves far from the sheltering walls of a friendly city, without a fleet. Their only recourse was to attempt a long, dangerous overland march through hostile territory. The Athenian general, Nikias, does his best to encourage them, despite their losses: they themselves are a city,

wherever they make camp. As they march, each man should imagine that 'the spot on which he is forced to fight, if he holds it, he will have his country and his walls'. If now they escape, the rest of them may have the fortune to see once more the object of their desire, while those who are Athenians 'will raise up again the great power of their city, though it has fallen. Men make the city, not walls or ships empty of men' (7.77). That this equation of men with the city is a cliché in no way diminishes its importance for understanding the role of ships and walls in Thucydides.[57] The statement confirms their importance, even while asserting the pre-eminent importance of people. The city's power may have fallen with the destruction of the fleet by the Syracusans; it can yet be salvaged, though, if the soldiers can be preserved. As Thucydides tells so vividly, however, with no walls or ships to protect them, the retreating Athenian army was virtually annihilated by the Syracusans.

It would be pleasant to have Thucydides' account of the end of the Peloponnesian war. Even so, we know what event marked the end for him: the confiscation of the walls and fleet of Athens. As he notes, he wrote (or intended to write) the history down to the time when the Lacedaimonians and their allies put an end to the Athenian empire, and took the long walls and the Peiraieus (5.26). The mark of the end of the war, in other words, is the destruction of Athenian power, as manifested by the confiscation of the city's walls and harbor. In fact, the historian Xenophon, who continues Thucydides history, provides a detailed account of the terms of surrender of Athens: in particular, they were required to abandon their fleet and tear down the long walls, which joined Athens to the Peiraieus (Xenophon, *Hellenica*, 2.2.20–23). It is tempting to imagine that, as Diogenes Laertius claimed, Xenophon wrote this section working from Thucydides' notes, and that his description of the destruction of the long walls represents something approximate to what Thucydides would have written: 'The Peloponnesians with great enthusiasm began to tear down the walls to the music of flute girls, thinking that that day was the beginning of freedom for Greece' (Xenophon, *Hellenica* 2.2.23).[58]

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[57] See C. Meister (1955) *Die Gnomik im Geschichtswerk des Thukydides* (Winterthur: 1955) 33.

[58] Diogenes Laertius 2.57. For Xenophon's continuation of Thucydides see e.g. Gomme, Andrewes and Dover (1981) 437–44.

Despite the overwhelming importance of walls and ships in Thucydides history, their meaning is problematic. As I have argued, the reason for this ambiguity is in the oscillation of the thing between utility and symbolic meaning. Things which are useful do not lie about their potential. To be recognized as tools, however, they must be read as signs of utility, and so they can lie. Thucydides is completely aware of the problem and, despite his own exploitation of ships and walls as tools of power, insists on it. This contradictory position, of course, risks discrediting the procedures of the history itself.

One of the most important instances of this tension between the instrumental and the meaningful comes in the context of Thucydides' discussion of the Trojan war, which has already been partially discussed above. Places like Mycenae, says Thucydides, are quite small by contemporary standards; even so, that is no justification to minimize the power which the place had. Thucydides proves his point by a contemporary example: if Sparta were layed waste, the paltry remains of the city would give an entirely misleading impression of the great power which it in fact possessed:

> Still, as the city is neither built in a compact form, nor adorned with magnificent temples and public edifices, but composed in villages after the old fashion of Greece, there would be an impression of inadequacy (1.10)

The phrase which Thucydides uses to describe the composition of Sparta 'in villages', κατὰ κώμας, is used consistently elsewhere in the history to describe settlements without walls. In fact, the expression is often coupled with the adjective 'unwalled', ἀτείχιστος (cf. 2.80; 3.94, 97; 4.70; (Aetolians); 3.101–2; 4.42, 43 (Solygia); 4.124).[59] If Athens, on the other hand were left in similar ruin, these traces would give the impression that its power was twice what it really was.

There are many things which distinguish Athens from Sparta: buildings, temples and the like. In particular, however, Athens has walls and Sparta does not. This difference does not mean that Sparta is weaker than Athens. As Thucydides specifically says, the appearance of things, even of what are by his own standards the most useful things, can lie.

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[59] I notice from Simon Hornblower's commentary on Thucydides (1991) 34–5, that he will discuss 'unwalled villages' in Thucydides in an article to appear in J. Sanders, ed. (forthcoming) *Philolakon: Lakonian Studies in Honor of Hector Catling* (Athens: forthcoming).

Like walls, ships can lie. In 416 the Athenians prepared an armada to conquer Sicily. This fleet, says Thucydides, 'was the most costly and splendid Greek force sent out by a single city up to that time'. In the number of ships, infantry, cavalry, in quantity and cost of the provisions and pay it equalled or exceeded all others known. In the ambitiousness of its purpose, no other was comparable. Still, the lavish expense of the armada itself called its meaning into question:

> From this [splendour] it was inferred (εἰκασθῆναι) among the rest of the Greeks to be more of a display (ἐπίδειξις) of power and resources than an armament (παρασκευή) against an enemy (6.31).

The word which Thucydides uses for display here, ἐπίδειξις, is interesting and significant. The word is used in other places in the history to describe demonstrations meant to impress rather than to accomplish anything. For example, at one time during the war, the Spartans attempted to foment the revolt of an Athenian ally, Lesbos. The Athenians, when they learned of this effort considered that it was due to the Spartans' belief of their own weakness. To show them their error, they sent a squadron to the Peloponnesus as a display (ἐπίδειξις), of their power (3.16). In another place, Thucydides tells how a Sicilian state, Egesta, sought a war alliance with Athens. The Athenians sent ambassadors to verify that the resources of the Egestaeans were sufficient to maintain their end of the alliance. The Egestaeans displayed (ἐπέδειξαν) their wealth in misleading way, so as to give 'an impression quite out of proportion to its really small value' (6.46).

CONCLUSION

Thucydides is well aware of the traditional, oral attitude toward objects and their link with memory, though he seldom avails himself of popular oral traditions relating to objects, and when he does he is often contemptuous.[60] He is even able to make an eloquent case for popular memory, and the words and actions by which memory is preserved. In the funeral oration, Thucydides represents a speech by the Athenian statesman, Perikles, delivered on the occasion of the funeral of those killed in the first year of the Peloponnesian war. Perikles remarks:

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[60] See notably his account of the tyranny of the Peisistratids, 5.54–59.

> For those who have sacrificed their bodies in common receive individually unaging praise (ἀγήρων ἔπαινον) and a distinguished tomb (τάφον ἐπισημότατον)- not so much the thing in which they lie, but that in which is preserved their renown (δόξα), to be eternally remembered on every occasion in word (λόγος) and deed (ἔργον). For the entire earth is the tomb of renowned men, and not only the inscription on the stones in their homeland indicates (στηλῶν σημαίνει ἐπιγραφή), but also abroad an unwritten memorial (ἄγραφος μνήμη ἐνδιαιτᾶται) resides in everyone, a memorial more of the meaning (γνώμης) than of the monument (ἔργου) (2.43).

The priority of memory lurks beneath Herodotus' and Plato's criticisms of written speech. They privilege oral speech because it appears to be more immediately linked to the consciousness of memory. Thucydides here is specifically concerned to have Perikles glorify memory against all forms of external representation, including specifically speech, objects, and that compromise of the two, writing. Significance, he has Perikles assert, is kept in memory, and can endure with no assistance from anything. Not only is memory independent of words, he asserts, but of things, monuments, as well. Nevertheless, he acknowledges a function for monuments, words and deeds. The dead have received 'a distinguished tomb - not so much the thing in which they lie, but that in which is preserved their renown' that is, their tomb—is not simply some inanimate monumental thing, but a tomb of memory. The glory of the dead is to be 'eternally remembered', summoned up 'on every occasion in word or deed'. Memory does not reside in the monument, any more than it does in words. The interaction between word and monument or action and monument, however, catalyzes memory, calls memory forth from dormancy.

The monument which Perikles speaks to dedicate is inscribed. As he specifically says, however, writing is no substitute for memory. Writing is limited and local. Memory spreads throughout the world, 'for the entire earth is the tomb of renowned men, and not only the inscription on the stones in their homeland indicates, but also abroad an unwritten memorial resides in everyone'. This last line is remarkable. It calls to mind a famous Platonic image, in which memory is described as a wax tablet on which one can write.[61]

The formulation which Thucydides puts in Perikles' mouth, like that which Plato puts into Socrates', is founded on a di-

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[61] See particularly the *Theaetetus*, 191 C–E and passim.

chotomy between inside and outside, self and alien, between human consciousness and the inanimate representation. In both cases, the possibility of an external meaning, autonomous of the person, is denied. This possibility or dichotomy, however, would not be imagineable in the absence of precisely some such external, autonomous meaning. The ability to discriminate between consciousness and thing, between reader and what is read, is dependent on the creation of some thing which has the apparent ability to sustain meaning outside of consciousness: a thing like writing.

The idea of writing transforms the imagined relationship of mind and thing from one of supplementary interdependence, a kind of coexistence, to a more mutually exclusive distinction. Herodotus and Thucydides are struggling between these attitudes. Likewise history and archaeology, in the modern sense of these terms, exist only by virtue of their reliance on the logic of writing: a separation of thing and consciousness, a conflict between observed and observer, subject and object. It remains to be seen if it is possible (or desirable) to think of things another way.

SPECIFIC MEDITERRANEAN FEATURES

GREEK HOROI: ARTIFACTUAL TEXTS AND THE CONTINGENCY OF MEANING

JOSIAH OBER

I AM THE *HOROS* OF THE AGORA

The Greek *horos* (boundary-marker) is a good starting point for a meditation on the social meanings that may be deduced by historians and archaeologists from the interrelationships between text and material artifact on the one hand, and between reference, performance, and social context on the other. In its simplest obviously recognizable form, the *horos* is a stone stele inscribed with the four letters ΟΡΟΣ.[1] It may thus appear to be a rather simple sort of artifact (a block of stone) carrying a rather simple sort of text (a word). Neither the artifact's form nor the text itself makes any claim to uniqueness. It can be iterated (copied or cited) by almost anyone in almost any context. At the same time, both the veracity and felicity of any given *horos'* claim to mark a boundary is a direct function of its specific location. The *horos* informs its reader that a border has been established at a particular point in space, and implicitly commands the reader to act accordingly. This command is an example of a written speech act in the form of a declarative statement: but what precisely is that statement?

Two narrow stone stelai, discovered still *in situ* during the excavations of the west side of the Athenian agora, make the *horos'* declaration explicit. Each stele is inscribed, in archaic

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[1] Examples (including such variants as ΗΟΡΕΣ [with three-or four-bar sigma], ΗΟΡΟ, ΟΡΟ, ΟΡΟϹ, [lunate sigma], ΟΡΟϹ Α' ['*horos* no. 1']): *Inscriptiones Graecae* I^2 902A, *Inscriptiones Graecae* II^2 2502–2524 (some 20 other examples are noted, but not actually printed, by the editors of *Inscriptiones Graecae*); J. Traill *Demos and Trittys: Epigraphical and Topographical Studies in the Organization of Attica* (Toronto 1986) 117, #7. As we will see, there are formally simpler types of *horos*; but these are not so easily recognized as *horoi*. I would like to thank Adrienne Mayor, Charles Hedrick, David Small, Merle Langdon, Greg Stanton, Hans Lohmann, and John Traill for comments and offprints which helped me to write a piece with which at least some of them will have profound disagreements.

lettering, with the following text: 'I [the stele] am the *horos* of the agora.'[2] On one level that sentence is a straightforward truth claim, a statement in the form of a declarative sentence that is either true or false. In order for the statement to be true (i.e. both accurate and coherent), the *horos* of the agora must remain fixed in its original position and the agora must exist. If someone were to pull up the agora *horos* and carry it somewhere else, the statement it proclaims would no longer be accurate, since it would no longer mark a boundary of the agora.[3] And if no one knew what 'the agora' was, the statement would cease to be coherent. Likewise, iteration is likely to entail a loss of veracity: for example, the statement that marks the beginning of this section of my paper is false (when read as a declaration) in that the page on which it appears is not the boundary of the agora.

The veracity of the *horos* is thus directly tied both to its physical situation and to the existence of an identifiable referent for the space that it claimed to delimit. Yet presumably no one (in ancient Athens or today) would understand the sentence, 'I am the *horos* of the agora,' as *merely* referential, i.e. as representing nothing beyond a banal statement of fact in the form 'x is y' (similarly, no reader of this article would be likely to take the section heading as a false referential claim being made by the page itself). In using the first person singular in order to proclaim itself 'the *horos* of the agora,' the stele was doing rather more than just informing a curious bypasser that 'this point just happens to be where the agora begins'; in asserting its presence in the first person, the *horos* takes for itself the role of watcher-over and guardian of the frontier it marked. As permanent watcher and guardian (one that remains present

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[2] ΗΟΡΕΣ ΕΙΜΙ ΤΕΣ ΑΓΟΡΑΣ: Agora inscription inv. nos. I 5510 (total height: 1.20 m., white island marble), I 7039 (original height: 0.68 m. above ground level), both dated to ca. 500 B.C. by context pottery and letter forms. A third, similar horos (I 5675) was found out of place. See H. A. Thompson and R.E. Wycherley (1972) *The Agora of Athens: The History, Shape and Uses of an Ancient City Center. The Athenian Agora* 14 (Princeton: 1972) 117–119; J.M. Camp (1986) *The Athenian Agora: Excavation in the Heart of Classical Athens* (London: 1986) 48–52; G. Daverio Rocchi, *Frontiera e confini nella Grecia antica. Centro richerche e documentazione sull' antichita classica. Monographie* 12 (Rome: 1988) 16.

[3] I am excepting here the case in which a legitimate authority moves the *horos* and reestablishes the boundary, e.g. in order to extend the space encompassed by the agora.

in the absence of its human author) the *horos* issued a command to the reader: 'The authorities order you not to cross the frontier unless you are are among those entitled to do so.' Thus, in the process of being informed that the agora begins 'right here,' the ancient Athenian reader of the agora *horos* was also enjoined to act in a certain way. If, for example, he had committed homicide, he was ordered to proceed no further: according to Athenian law, murderers (and certain other malefactors) were forbidden to enter the agora.[4] The *horos* is thus an example of what some speech-act theorists call the performative-constative: that which is grammatically a statement of fact (a truth claim with a referentially clear subject) is also an action. That is, to say, the statement does something, it performs a significant and obviously recognizable social function.[5]

The question one usually asks of a performative statement is not whether it is true or false, but whether it is felicitous or infelicitous, i.e. whether the intended speech act (in this case, an order) was efficaciously delivered and received—whether the 'uptake' was successful (this is, of course, not the same as asking whether or not the order was in fact obeyed).[6] Achieving felicity is a matter of context. It requires that the parties

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[4] The Greek term for 'watcher-over' or 'guardian' is οὖρος, formally identical to the Ionian from (used by Homer) for ὅρος and often used in compounds related to the protection of liminal zones, e.g. πυλωρός (gate-keeper), θυρωρός (door-keeper), φρουρός (guard); even if, as is generally supposed, the terms (ὅρος, οὖρος) do not share a common root, the concepts (border/frontier, watcher/guard) are intimately related in Greek practice and thought. On 'egocentric' (first person) inscriptions and their assertion of presence, see J. Svenbro (1988) 'J'écris, donc je m'efface. L'énonciation dans les premieres inscriptions grecques,' in *Phrasikleia: Anthropologie de la lecture en Grèce ancienne*, ed. J. Svenbro (Paris: 1988). Laws limiting access to the agora: Andocides 1.71, 76; Aeschines 3.176; Demosthenes 24.60; cf. D.M. MacDowell (1975) *The Law in Classical Athens* (Ithaca: 1975) 111; Camp (1986) 51.

[5] For a cogent introduction to speech act theory, see S. Petrey (1990) *Speech Acts and Literary Theory* (New York: 1990), who points out (42–56, contra E. Benveniste) that a speech act need not take the specific grammatical form of an imperative; cf. J.R. Searle (1969) *Speech Acts: An Essay in the Philosophy of Languages* (Cambridge: 1969) 68. On commands or orders as speech acts: Searle (1969) 64, 66.

[6] The distribution between felicity and obedience is clarified by considering the order-issuing authority's reaction to a failure to obey. That reaction can be quite different if the authority in question believes that uptake occurred (i.e. that the person given the order understood the order and the relevant social rules) or did not occur (i.e the person ordered failed to grasp the sense of the order). An example of the first (felicitous) case

in question—she who gives the order and he who receives it—be willing to operate according to the same general set of social conventions. Furthermore, the receiver of the order must accept the legitimacy of the authority giving the order.[7] Not every speech act which takes the form of a declarative sentence requires that the statement be true in order to achieve felicity.[8] But in the case of the *horos*, the felicity of the speech act does indeed seem dependent on its veracity, that is upon its being both properly located and referentially coherent.

The relatively prolix (four-word) agora *horos* informs its readers of the name of the space it delimits; moreover, because the Athenian agora was public space, the inscription clearly implies that it was established and backed up by public authority (i.e. by the government of the polis of Athens). Thus anyone who knows the rules associated with the agora will understand how he or she is expected to act and by what body he or she might be expected to be punished for misbehavior. The simpler (one-word) form of *horos*, described in the first paragraph of this article, does not name what it delimits or who authorized it; rather it assumes that its readers will be able to supply those names and the associated rules for themselves. For the knowledgeable member of the society that established it, the one-word *horos* says a good deal: 'I am the boundary of "x", established by the legitimate authority "y", and you are accordingly ordered to act in the prescribed manner "z".' The fundamental interpretive problem presented by the laconic one-word *horos* is how we, not being members of ancient society, are to fill in the values of 'x', 'y', and 'z'.

The problem of interpretation is compounded by the possi-

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is a licensed motorist ignoring a stop-sign. An example of the second (infelicitous) case is a recent arrival to the state of Montana crossing a fence marked with orange paint (indicating 'no trespassing'). While it may be that, legally speaking, 'ignorance of the law is no excuse,' in ordinary social practice the distinction is frequently highly relevant.

[7] Conditions for felicity: J.L. Austin (1975) *How to Do Things with Words*, ed. J.O. Urmson and M. Sbisà (Cambridge MA: 1975) 14–45; Searle (1969) 62–71; Petrey (1990) 12–13.

[8] For example, a gangster boss says in the presence of his henchmen, 'X is a dead man.' His minions answer, 'Right, boss.' In actuality X is, at the moment, alive and well; both the boss and his henchmen know this. Yet the boss's statement is obviously not a false description, but a command to murder X. The henchmen do not aquiesce to a false description of reality, but indicate their intention to obey an order. The boss's statement is a felicitous (if reprehensible) speech act because the henchmen grasp the boss's meaning and accept his authority.

bility that the one-word *horos*—as a single artifact bearing a single, unaltered text—could, in the course of its existence, quite easily come to delimit referentially different spaces. And it could thereby come to demand completely different sorts of behavior of its reader. Suppose, for example, that a man marked the border of his privately owned plot of land by erecting a *horos*. After his death, his son dedicates the land to a god. In this case the *horos*, which once marked the border of a private field, now marks the border of a sacred temenos. Alternatively, a one-word *horos* stone might be removed from its original position (e.g. marking private land) and then formally installed in a different position (e.g. to mark an existing sacred temenos). In either case, a text which originally signified and performed the sentence, 'I am the *horos* of private land; I order you not to trespass,' later signified and performed a different sentence: 'I am the *horos* of the sacred temenos; I command you to perform the appropriate ritual.' Exactly the same physical object, exactly the same text; and yet the former values 'x' and 'z' were quite easily changed to 'a' and 'c.' The one-word *horos* thus provides a particularly clear example of the impossibility of complete authorial control of textual meaning: the individual who cut the original *horos* had no control over its subsequent meanings in the two hypothetical cases just noted. And thus, even if modern archaeologists can assign particular 'x', 'y', and 'z' values with a high degree of probability to a given *horos*, we must not suppose that they have therefore isolated *the* unitary and permanent meaning of the sign.[9]

Below, I will look at several examples of ancient Greek *horoi*, beginning with some ancient literary descriptions of artifacts unattested archaeologically and concluding with several series

[9] *Horoi* marking sacred lands: *Inscriptiones* I² 858, 860; II² 2596–2612 (e.g. ΟΡΕΣ ΙΕΡΟ, ΟΡΟΣ ΤΕΜΕΝΟΥΣ, with or without name of the relevant deity added). Historical mid-fourth century B.C. examples of erecting *horoi* to mark as sacred land which had formerly been cultivated privately: By officials of the Amphictyonic League in lands claimed by Amphissans (decree apud Demosthenes 18.154: καὶ στήλαις διαλαβεῖν τοὺς ὅρους). By the Athenians in the territory known as Orgas on the Megarian border (*Inscriptions Graecae* II² 204); cf. J. Ober (1985) *Fortress Attica: Defense of the Athenian Land Frontier, 404–322 B.C.* (Leiden: 1985) 216 n.16; G. Daverio Rocchi (1987) 'La ἱερὰ ὀργάς e la frontiera attico-megarese', in *Studi di Antichità in Memoria di Clementina Gatti* (Milan: 1987) 97–109. For a stimulating treatment of the diverse 'social meanings' of the *horos* as a sign, and the 'plasticity' of horos as a symbol, see L. Gernet (1981) 'Mortage *Horoi*' in *The Anthropology of Ancient Greece* eds. and trans. J. Hamilton and B. Nagy (Baltimore: 1981 [1955]) esp. 309.

of *horoi* discovered by field archaeologists, artifacts for which there is no secure literary attestation. I hope to make two main points: first, the *horos* is a marker of socially vital distinctions; next the nature of the distinctions in question is contingent on a knowledge of changing and historically specific social codes. Looking at how several texts treat *horoi*, and examining the *horos* as an 'artifactual text'—as a text that is an artifact, and at the same time an artifact that is a text— should help to elucidate some methodological problems involved with moving back and forth between texts and artifacts, history and archaeology. Along the way I hope to demonstrate that problems addressed by literary theorists and philosophers of language are not necessarily clever-but-irrelevant word games; certain of these problems can have direct bearing on the everyday work of historical and archaeological interpretation.

ATHENA'S STONE: *HOROS* OF THE PLOWLAND

In book 21 (403–405) of the *Iliad*, in the midst of a fight with the god Ares, the goddess Athena picks up a stone which, as Homer informs us, was 'lying there on the plain, [it was] dark, rough, and huge; former men had established it as a boundary of the plowland.'[10] The brief reference does not, at first glance, give the reader a great deal to go on: The stone's great size (*megan*) points to Athena's superhuman physical strength. And the reference to 'former men' (*andres proteroi*) and their artifacts has obvious parallels with other poetic references to the ancient residents of the Troad and their artifactual remains.[11] Athena's stone plays a minor role in the epic, but is a valuable starting point for investigating a *horos* in a literary text as a complex archaeological artifact.

The stone in question was rough (*truchên*) and so appar-

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[10] ἡ δ' (ἀναχασσαμένη) λίθον εἵλετο χειρὶ παχείῃ/ κείμενον ἐν πεδίῳ μέλανα, τρηχύν τε μέγαν τε,/ τόν 'ρ ἄνδρες πρότεροι θέσαν ἔμμεναι οὖρον ἀρούρης·/ τῷ βάλε θοῦρον Ἄρηα κατ' αὐχένα, λῦσε δὲ γυῖα. "But she [Athena], drawing back, seized a stone in her mighty hand; it was lying there on the plain, dark, rough, and huge; former men had established it as a boundary of the plowland. With this she struck furious Ares on the neck, loosing his knees.'

[11] E.g. 2.811–815 (the poet mentions a steep mound which men call Batieia, but the gods know as the *sêma* of Myrine); 23.326–333 (Nestor points out to his son a dead stump flanked by stones which he posits may either be the *sêma* of a long dead man or a race marker established by 'former men' (ἐπὶ προτέρων ἀνθρώπων).

ently unworked, ergo a natural object, rather than an artifact. Yet it had been 'established' (*thesan*) as a *horos* by the act of men. What precisely were the human actions (words or rituals) which constituted the establishment? We cannot say. It is perfectly possible to suppose that the men of old had physically moved the stone from somewhere else to the edge of the plowland. And thus the very act of setting it into place could have constituted its establishment (thus *LSJ* s.v. τίθημι, A.). But it is equally possible to suppose that the plowland itself was laid out only as far as the location of a stone that was already lying (*keimenon*) in place. In this case some pronouncement or ceremony presumably would have been performed (*LSJ* s.v. τίθημι, B.) in order to establish as *horos* a stone that remained *in situ*. The status of the stone as an artifact seems somewhat less problematic in the first case than in the second. Yet, unless the stone were part of a recognizable series (i.e. either built into a continuous wall or one of a line of similar stones), there would be nothing about this very stone to distinguish it as as a holder of special meaning. To 'later people' (let us say, modern field archaeologists surveying the Trojan Plain), Athena's stone would not be recognizably a *horos*, but just another big hunk of rock. Homer's readers know that the stone Athena picked up was once a *horos*, but only because he informs them of this fact; the poet's text imparts meaning to an otherwise unremarkable object. But the text does not not enable its readers to move from the stone mentioned in the text to any given material stone visible in the field: The *Iliad* passage may alert the archaeologist surveying the Troad to the possible function of actual 'dark, rough, huge' stones, but it does not *eo ipso* allow him/her to establish the function of any given stone.[12]

The Homeric passage underlines the contingency of signification: The 'former men' knew the stone was a *horos* because they operated according to an agreed-upon common code of signs and behavior (a language of words and actions) and were participants (even if indirectly) in the act which established the stone as a boundary. Once they were gone, the code was lost (except to knowledgeable poets) and so the once-established meaning slipped from the object. Later people (let us

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[12] Unless, of course, the geology of the region were such that the appearance of big, dark, rough stones on the surface could confidently be assigned to human agency.

say, archaeologists) attempting to reconstruct the code used by the former men will not be able to restore the earlier meaning unless they are lucky enough to have available a text that is quite specific about the location and appearance of the object. In this case, Homer's text takes us part of the way, but not quite far enough.

Homer does name the space delimited by the *horos*: he tells us that the stone once marked the boundary of the plowed land. Thus the natural object, once it had been established as an artifact, differentiated the land that was cultivated from that which was not. Why had the former men thought it necessary or desirable to mark this distinction by establishing a *horos*? Once again, the text fails us. The poet does not indicate whether the boundary had a significance that was religious: was the wild land sacred to some deity? Or economic: was the uncultivated land owned by someone other than the owner of the cultivated land, and/or put to some different economic use (e.g., as grazing grounds)? Nor does he identify the authority by which the stone was established as a boundary: the decree of a king? an agreement among family groups living in the area? a divine pronouncement? We cannot even say whether the authority was sacred or secular, public or private. Moreover, lacking any further textual elucidation, we cannot say what the intended practical effect of the existence of the boundary would have been, that is to say, how the behavior of the 'former men' would have been altered after the *horos* had been established: were only certain persons given (or denied) permission to pass the boundary? Was it forbidden to cultivate land beyond it? We don't know. And a fortiori we have no idea what sanctions might have been imposed upon those who chose to ignore the boundary marker or whose responsibility it was to enforce the sanctions.

Homer's text does, however, make one further and very important point about the boundary-stone: if (per above) its meaning as a boundary was not fixed for all time, nor was its physical location. The marker could be and was moved (albeit by divine force). Through Athena's action in grabbing it up, the stone was clearly stripped of any remnant of its former meaning as a boundary. After it had been used to bash Ares, the stone was no longer *in situ* and so could no longer be regarded as marking a boundary (existing or historical) even by those who had been informed by a poet of its (former) status. By employing the stone as a weapon, Athena in effect

annulled its claims in regard to the delineation of space and returned the human artifact to the status of natural object. And thus, even the field archaeologist capable of locating the edges of ancient field systems would not recognize the displaced rock as a *horos*.

The fact that Homer felt it was worthwhile to mention that Athena's stone had once been a *horos* points to the significance attached to the establishment of physical boundaries in Greek culture.[13] To the members of the society that had installed it, the *horos* proclaimed that certain territory was 'this, not that'—it marked a difference and enacted a distinction where distinction had not previously existed. And thus the establishment of a *horos* was a way of imposing human, cultural, social meanings upon a once-undifferentiated natural environment. The land was brought under human economic control by the work of cultivation and under semiotic control by the closely associated act of instituting markers. These markers had a functional purpose: they indicated different uses to which 'this land' could properly be put as opposed to 'that land' on the other side of the established boundary. Moreover, the distinctions thus marked had social ramifications: as signs of human control, markers could have the effect of establishing not just what the land could be used for, but who had the right to use, perhaps even to tred upon, the demarcated terrain.

If *horoi* were centrally important symbolic and social markers, then it mattered a great deal who had the power to establish or to disestablish them. Possession of the authority to make or unmake distinctions, to attach new and widely accepted significance to objects (e.g. unworked stones), is surely one of the most fundamental measures of human power.[14] This is particularly clear when the decision to create a distinction has obvious economic effects. In complex human societies, determining who can use parcels of arable land and for what purposes, has immediate and obvious economic implications. So historians and archaeologists certainly do want to know the nexus of human relationships and decisions that established *horoi* point to. But Homer's account of Athena's stone and its

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[13] Boundaries are, of course, important in other cultures as well; see e.g. *O.T.* Genesis 31.44–48, Jos. 22.26ff., Is. 19.19–20; and in general Green and Perlman (1985); M.Z. Ispahani (1989) *Roads and Rivals: The Political Uses of Access in the Borderlands of Asia* (Ithaca: 1989).

[14] See J. Ober (1989) *Mass and Elite in Democratic Athens: Rhetoric, Ideology, and the Power of the People* (Princeton: 1989) 338–39.

former status also reminds us that the *horos* itself may be able to tell us next to nothing about those relations and decisions; indeed we may not even know a *horos* when we stumble over it in the field.

SOLON: *HOROS*-ANNULLER AND *HOROS*

The contingency of the social meanings proclaimed by *horoi*, and the problems of historical interpretation raised by that contingency, become very clear when we turn from the mythic world of Homer to the historical figure of the Athenian magistrate, Solon. In a poetic passage listing his accomplishments as *archon*, quoted by *Ath. Pol.* (12.4 = Solon F 36), Solon speaks of the promises he was able to fulfill by exerting his own power (*kratei*) and by bringing to bear force (*bia*) combined with justice (*dikê*).[15] He asserts that if the extent of his reform program is ever brought into question, the black earth, supreme mother of the Olympians, will be his witness at the tribunal of Time: 'For on her behalf I annulled (*aneilon*) the *horoi* which had been established everywhere, so that being formerly enslaved, she is now free.'[16] Solon's claim to have acted as disestablisher of *horoi* and liberator of the earth is closely connected in his poem to another liberation: the freeing (*eleutherous ethêka*) of Athenians: the repatriation of those Athenians who had been forced into exile or sold abroad as slaves and the freeing of those who remained in Attica but were forced into a slavish condition and trembled at their master's every whim.[17]

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[15] ταῦτα μὲν κράτει,/ ὁμοῦ βίαν τε καὶ δίκην συναρμόσας,/ἔρεξα καὶ διῆλθον ὡς ὑπεσχόμην (lines 15–17 'These things I achieved by my power, harnessing together force and justice; and I persevered in my promises' [trans. Rhodes]). I am following the reading of the Berlin papyrus, adopted by Linforth, Rackham (Loeb ed.), and Chambers (Teubner ed.) rather than that preferred by Kenyon (Oxford ed.): μὲν κράτει/ νόμου, βίαν.... For discussion of this important crux, see M. Ostwald (1969) *Nomos and the Beginnings of the Athenian Democracy* (Oxford: 1969) 3 n. 5: P.J. Rhodes (1981) *A Commentary on the Aristotelian Athenaion Politeia* (Oxford: 1981) 176; G.R. Stanton (199) *Athenian Politics c. 800–500 B.C. A Sourcebook* (London and New York: 1990) 56 n. 5.

[16] συμμαρτυροίη ταῦτ' ἂν ἐν δίκῃ χρόνου/ μήτηρ μεγίστη δαιμόνων Ὀλυμπίων/ ἄριστα Γῆ μέλαινα, τῆς ἐγώ ποτε/ ὅρους ἀνεῖλον πολλαχῇ πεπηγότας·/ πρόσθεν δὲ δουλεύουσα, νῦν ἐλευθέρα (lines 3–7 'The witness of these things in the courtroom of Time is the greatest and the best Mother of the Olympian gods, black Earth, from which I pulled up the *horoi* which were fixed in many places; so that formerly enslaved, now she is free' [trans. Rhodes, adapted].)

[17] πολλοὺς δ' Ἀθήνας πατρίδ' εἰς θεόκτιτον,/ ἀνήγαγον πραθέντας, ἄλλον

Solon pointedly contrasts (through a *men/de* construction) these acts of liberation— accomplished by power: force combined with justice—to his writing of laws (*thesmous... egrapsa*) that would be applied equally to bad and good alike.[18]

Despite enduring academic uncertainties about Solon's precise constitutional position, the interrelationship of political power, authority over *horoi,* and changes in social relations are explicit in this fragment. Solon freed the earth by annulling *horoi,* just as he freed the Athenians by announcing their liberation. In both cases, previously existing distinctions were declared invalid. Solon's action meant that Athenians would no longer be subdivided into separate status categories of free and slave; to be an Athenian (*male*) now entailed being a free person, a citizen.[19] And whatever distinctions, marked by *horoi,* that had formerly pertained in respect to use of the 'black earth' were now eliminated. But what distinctions did the pre-Solonian *horoi* mark? Even without knowing as much as we might like about the conditions of archaic slavery, we may hope to explain in general terms what it might have meant for an individual Athenian to be enslaved or to be freed. It is much more difficult to guess what it meant for 'black earth' to be in a condition of slavery or freedom. It is commonly assumed by scholars that the *horoi* annulled by Solon were written records of indebtedness or encumbrance, more or less similar in function, and perhaps in form, to well-attested (archaeologically

ἐκδίκως,/ ἄλλον δικαίως, τοὺς δ' ἀναγκαίης ὑπὸ/χρειοῦς φυγόντας, γλῶσσαν ὀυκέτ' Ἀττικὴν/ ἰέντας, ὡς ἄν πολλαχῇ πλανωμένους·/ τοὺς δ' ἐνθάδ' αὐτοῦ δουλίην ἀεικέα/ ἔχοντας, ἤθη δεσποτῶν τρομευμένους,/ ἐλευθέρους ἔθηκα. (lines 8–15 'To Athens, to their divinely-founded homeland, I brought back many persons who had been sold, some unjustly some justly; some of these had fled out of dire necessity, they no longer spoke the Attic tongue after wandering in my places; and others who were subjected here to shameful slavery, fearing the whims of their masters, I made into free men' [trans. Rhodes, adapted].)

[18] ταῦτα μὲν κράτει ... θεσμοὺς δ' ὁμοίως τῷ κακῷ τε κἀγαθῷ/ εὐθεῖαν εἰς ἕκαστον ἁρμόσας δίκην,/ ἔγραψα. (lines 15–20 'These things, on the one hand [I accomplished] by my power ... but on the other hand, I wrote laws for good and bad people alike, providing straight justice for each man' [trans. Rhodes, adapted]). Following the Berlin papyrus and the reading of Linforth rather than Kenyon and Chambers, who read θεσμούς θ' .... See Rhodes (1981) 177.

[19] Association of the sign of the *horos* with power over persons and property: Gernet (1981) 309. Solon and citizenship: K. Raaflaub (1985) *Die Entdeckung der Freiheit: Zur historischen Semantik and Gesellschaftsgeschichte eines politischen Grundbegriffes der Griechen. Vestigia: Beiträge zur alten Geschichte* 37 (Munich: 1985) 62–65; Ober (1989) 60–65; P.B. Manville (1990) *The Origins of Citizenship in Ancient Athens* (Princeton: 1990) 124–56.

and in literary sources) inscriptional 'security *horoi*' of the fourth century B.C., which were quite detailed records cut into stelai, set up on land that was used as collateral to secure a loan. These fourth-century inscribed records publicly proclaimed the terms under which the property they demarcated was hypothecated. If the Solonian-era *horoi* were official land-encumbrance records of some sort, the annulment of *horoi* would mean the annulment of outstanding debt (or at least of the requirement to pay over some part of the crop annually) and so presumably the annulment would be part of (or equivalent to) the *seisachtheia* ('shaking of off burdens') mentioned by *Ath. Pol.* (6.1, 12.4) and Plutarch (*Solon* 15.3–5, 16.3). The presumptive beneficiaries of the annulment of *horoi* would have been the poorer Athenian peasants, the persons Plutarch (Solon 13.2) says were called *hektêmoroi.*[20]

This scenario may well be generally correct, but it is complicated by the intractable problem of reconstructing archaic notions of 'ownership' and 'alienability'; moreover it goes considerably beyond what Solon himself says he did. As Linforth rightly pointed out, 'his [Solon's] statement is cast in a poetical form, sufficiently clear for his readers, who knew exactly what he was referring to, but somewhat obscure to us.'[21] That obscurity is intensified by the fact that no archaic artifact has ever plausibly been identified as a *horos* of the sort Solon claims he annulled; there is no archaeological evidence for hypothecation stelai from Attica earlier than the late fifth century. This apparent gap in the archaeological record could be explained by supposing that the *horoi* in question were made of perishable material.[22] But given the apparent absence of

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[20] Fourth-century hypothecation *horoi*: J.V. Fine (1951) *Horoi: Studies in Mortgage, Real Security, and Land Tenure in Ancient Athens. Hesperia Supplement* 9 (Princeton: 1951); M.I. Finely (1952) *Studies in Land and Credit in Ancient Athens: 500–200 B.C.* (New Brunswick: 1952); P. Millett (1991) *Lending and Borrowing in Ancient Athens* (Cambridge: 1991). Solonian *horoi* in relationship to fourth-century hypothecation *horoi* and the link to the *seisachtheia* and the *hektêmoroi*: F.E. Adcock (1926) in *Cambridge Ancient History* (1st ed. 1926) II.33–38; A. Andrewes (1982) in *Cambridge Ancient History* (2nd ed. 1982) III(2) 377–382; I. Linforth (1919) *Solon the Athenian* (Berkeley: 1919) 62–66; Gernet (1981) 303–11; Manville (1990) 126–28. Earlier literature on the subject: Finley (1952) 199 n. 23.

[21] Linforth (1919) 62. Cf. Andrewes (1982) 377: 'His [Solon's] hearers knew in detail what he meant but we have to guess, and the word *horoi* does not by itself settle the issue . . . [But] Solon cannot be saying simply that he abolished many boundaries.'

[22] Theory of wooden *horoi* to explain the fact that no Solonian-era *horoi*

evidence for wooden *horoi*, the absence of Solonian-era *horoi* from the archaeological record might more easily be explained by the assumption that the *horoi* in question were unworked and anagraphic stones, similar in form to the *horos* of the plowland mentioned by Homer. If this is correct, the Solonian *horoi* may actually still exist and some may even be visible in the fields of Attica, but they are no longer recognizable as *horoi*. Yet if the *horoi* annulled by Solon were unworked, uninscribed stones, they were evidently markers of distinction rather than detailed records of financial transactions. So what distinction might they have marked before Solon's action? The context of the 'black earth' passage surely implies that the annulment was linked to the establishment (or restoration) of just relations between a powerful Athenian elite (the men Solon calls *meizous kai bian ameiones*: Ath. Pol. 12.5 = Solon F 37.4) and the Athenian masses (whom he calls the *dêmos*: F 36.22, F 37.1). As an alternative to the 'record of indebtedness/encumbrance' thesis, we might posit that before Solon's action the *horoi* had marked areas of Attica in which the movements of Athenians of a certain status (e.g. *hektêmoroi*, the enslaved, or non-Eupatrids) were restricted (e.g. areas which they could not leave or enter). This is all, of course, mere speculation. It is offered only to show that assuming the pre-Solonian *horoi* to have been financial records of hypothecation or encumbrance is also speculative and that internally coherent alternative hypotheses can be offered. In the end, we may be forced to admit that we do not know what a pre-Solonian *horos* would have looked like or precisely what it signified; as in the case of Athena's stone, the meaning of the marker is contingent upon a vanished (or, in this case, abolished) matrix of social relations and rules. We simply do not know enough of the social code of pre-Solonian Athens to reconstruct meanings that were presumably easily accessible to most members of early sixth-century Athenian society.

Solon uses the word *horos* metaphorically in another fragment (37) cited by *Ath. Pol.* (12.5) in discussing his relationship with the two parts (*dêmos* and powerful men) of Athenian society. He claims that whereas someone else in his position would not have restrained the common people nor rested before

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have ever been found: Stanton (1990) 56. Cf. Adcock (1926) 33–34: 'there is no reason to assume that the [boundary] pillars were of stone indelibly inscribed.'

his agitation had skimmed the butter from the milk, 'I set myself between them, like a *horos* in disputed terrain.'[23] Here the man who elsewhere boasted of having removed distinction-establishing *horoi*, proudly takes for himself the name and the distinction-enforcing role of the *horos*. Rather than being a devisive instrument for the enslavement of that which should be free, here the (metaphorical) *horos* establishes the vital and legitimate distinction that prevents unfair treatment of one social group by another. One implication of Solon's self-description as a *horos* is that the problem he claims to have solved in the first passage was not the result of distinction *ipso facto*, but rather of the wrong sort of distinction. We may remember that Solon not only freed enslaved Athenians, but established (or at least revised) precisely defined, wealth-based status categories (*telê*) which officially determined the distribution of political powers and responsibilities for all Athenian citizens.[24]

Moreover, Solon's legislation forbidding the future enslavement of Athenians by their fellows may be regarded as the origin of the formal status of the citizen: an impassable *horos* was (metaphorically) erected between the categories of 'free Athenian' and 'Athenian slave.' It is very likely that Solon's permanent ban on the enslavement of Athenians took the form of a written *thesmos*.[25] If so, it was distinct in Solon's mind and

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[23] F 37: οὐκ ἂν κατέσχε δῆμον οὐδ' ἐπαύσατο,/ πρὶν ἀνταράξας πῖαρ ἐξεῖλεν γάλα./ ἐγὼ δὲ τούτων ὥσπερ ἐν μεταιχμίῳ/ ὅρος κατέστην. 'He would not have restrained the people nor have stopped until he had stirred up the milk and skimmed off the cream. But I set myself between them, like a *horos* in the contested terrain between the spears' [trans. Rhodes, adapted].

[24] Wealth categories: *Ath. Pol.* 7.2–4; Plutarch, *Solon* 18.1–2. T.C. Stinton, (1976) 'Solon, Fragment 25', *Journal of Hellenic Studies* 96 (1976) 159–62, bothered by Solon's self-description as *horos*, argues that ὅρος should be emended to οὖρος (watcher/guardian), which seems to me quite unnecessary; cf. above, n. 4 on the closeness of the two terms. Rhodes (1981) 179, after discussing various proposed emendations (and noting that ὅρος and οὖρος may be the same word), concludes 'I am not sure that the explanation of this sentence has yet been found.' But cf. Manville (1990) 126–27: 'The metaphor of the boundary stone was certainly apt. For a general theme in all of Solon's reforms was the creation of boundaries—spatial, legal, and even psychological.' N. Loraux (1984) 'Solon au milieu de la lice', in *Aux origines de l'Hellénisme: La Crète, et la Grèce. Hommage à Henri van Effenterre* (Paris: 1984) 199–214, rightly emphasizes the competitive/military connotations of 'being in the middle of things' in this fragment rather than the connotation of political 'moderation.'

[25] *Ath. Pol.* 6.1 mentions forbidding human collateral for debts, establishing *nomoi*, and the *seisachtheia* as three distinct reforms; at 10.1 forbidding human collateral is included in Solon's *nomoi*, and the cancellation is

poetic description from the original liberation itself, which had been accomplished (according to the fragment quoted above) by personal power and 'force combined with justice.' Thus power (*kratos*) and force (*bia*) could justly be employed to disestablish illegitimate distinctions; but the establishment of new distinctions through the production of law is described by the poet-statesman simply as 'writing' (*thesmous*... *egrapsa*). The differentiation between the conditions he thought appropriate for performing the two varieties of speech act may cast some light on how Solon viewed (at least in retrospect) the role he had played in the restructuring of Athenian society, and how he understood the nature and extent of the powers he had temporarily wielded.[26] The term Solon uses for 'disputed terrain,' *metaichmion*, refers literally to the space between the spears of opposing armies. Yet clearly Solon's use of the word is metaphorical: setting a *horos* between two armies would be nonsensical, whereas establishing a boundary in a disputed frontier zone is a reasonable and understandable way of preventing destructive conflict. Solon's metaphorical reference to his own political position as a *horos*, marking a disputed frontier, points to the actual role of physical *horoi* in establishing the exterior territorial borders of a self-contained society (a polis), as well as in establishing valid distinctions operative within a society.[27]

HOROI OF THE *PATRIS*

Sometime in the archaic period, perhaps even before Solon's reforms, the Athenian ephebes, eighteen- and nineteen-year-old Athenian soldiers-in-training, began to swear a standard

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said to be before his period of *nomothesia*. Plutarch, *Solon* 15.3, lumps these altogether under the rubric of the *politeuma* which Solon wrote. Cf. Linforth (1919) 65–66, who considers forbidding human collateral to be a formal law and separate from the liberation itself.

[26] It is tempting to suppose that Solon felt that a tyrant was one who used *kratos* and *bia* (rather than the writing of *thesmoi*) to establish new social distinctions. Such a description would certainly suit sixth-century tyrants such as Kleisthenes of Sicyon (Hdt. 5.67–68).

[27] The existence of a recognized border between *poleis* is indicated by the ritual of offering border-crossing sacrifices (διαβατήρια), particularly associated with the Spartan army: D.J. Mosley, 'Crossing Greek Frontiers in Arms,' *Revue internationale des droits de l'antiquite* 3.20 (1973) 161–169; M.H. Jameson (1991) 'Sacrifice before Battle', in *Hoplites: The Classical Greek Battle Experience*, ed. V.D. Hanson (London and New York: 1991) 198 with n. 2, 202, 220, and reference cited. See also below n. 33.

oath of allegiance to their fatherland (*patris*).[28] According to the preserved fourth-century B.C. copy of the archaic oath, the ephebe swore to respect sacred rites, to obey the laws and those magistrates who acted rightly, not to dishonor his weapons, not to abandon his place in the line, and to fight alongside his comrades in defense of all national assets, sacred and secular. Moreover, 'I shall not hand down a lessened *patris*, but one that is increased in size and strength, [striving] myself and along with all the others.'[29] The oath itself is followed by a long list of witnesses (*histores*): 'gods, Agraulos, Hestia, Enuo, Enualios, Ares and Athena Areia, Zeus, Thallo, Auxo, Hegemone, Herakles; *horoi* of the *patris*, wheat, barley, vines, olives, figs.'

What did the ephebes and those administering the oath have in mind as a referent for the words *horoi tês patridos* (boundaries of the fatherland)? The grammar of the second part of the list of witnesses is not absolutely clear. The witness list begins with the term '*theoi*' (gods). The gods in question are then named: a total of eleven divinities, including personifications of 'increase, growth, and leadership' (Thallo, Auxo, Hegemone). So far so good. The second part of the list begins with the term '*horoi* of the *patris*' and continues with five major products of arable agriculture. And here lies the crux: Is this part of the list paratactic: a series of (six) separate witnessing entities, one of which is '*horoi*'? Or is the phrase *horoi tês patridos*, like *theoi*, a collective noun whose elements are described by the (five) words that follow? On the second hypothesis, we might suppose that 'wheat, barley, vines, olives, figs' is a collective metonym for 'the *horoi* of the fatherland.' Or (on an even closer parallel with *theoi*) that among a larger set of *horoi* tês patridos, the particular *horoi* that will witness the oath are (again metonymically) the several products. According to the first (paratactic) hypothesis, the *horoi* are not defined as agricultural products; their physical form remains textually indeterminate. According to the second (metonymic) hypothesis, the

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[28] M.N. Tod, ed. (1948) *A Selection of Greek Historical Inscriptions* (Oxford: 1948) II.204 (= P. Harding, ed. (1985) *From the End of the Peloponnesian War to the Battle of Ipsus. Translated Documents of Greece and Rome* (Cambridge: 1985) II.109), a fourth-century inscription from the Attic deme of Acharnai which preserves the oath. For its archaic character (pre-Peisistratid and perhaps pre-Solonian) and discussion of earlier literature: P. Siewart (1977) 'The Ephebic Oath in Fifth-Century Athens,' *Journal of Hellenic Studies* 97 (1977) 102–11.

[29] ο<ὐ>κ ἐλάττω παραδώσω τὴν πατρίδα, πλείω δὲ καὶ ἀρείω κατά τε ἐμαυτὸν καὶ μετὰ ἁπάντων (lines 10–12).

horoi themselves are actually agricultural products.[30] The metonymic reading certainly makes for better syntactic parallelism (*theoi/horoi*). Moreover, according to Plutarch (*Alcibiades* 15.4), a metonymic reading was adopted as early as the late fifth century by the politician and general, Alcibiades, who supported his policy goal of boundless imperialism with the argument that the Athenians had sworn to treat as their own *patris* any place in which wheat, barley, vines, olives, and figs were grown. But we need not accept Alcibiades as an unimpeachable authority for the original, archaic reference; his manipulation of the language of the oath falls squarely within the sophistic tradition of twisting accepted meanings to support otherwise dubious positions.[31]

Supposing that wheat, barley, etc. actually are *horoi* requires that we accept a set of referents for the term *horoi* that is at odds with the referents implied by the Homeric and Solonian passages discussed above. Yet even on a paratactic reading, the syntax of the witness list obviously implies a close relationship between the *horoi* of the fatherland and the various agricultural products. This relationship is hardly unexpected: 'Athena's stone' marked the border of the plowland and the *horoi* Solon annulled to free the black earth certainly had something to do with the use of arable land. As an alternative to a purely metonymic or purely paratactic reading, one might be tempted to try a metaphorical compromise: the wheat, barley, vines, olives, and figs refer metaphorically to the actual stone boundary-markers which delimit the fields in which these products were grown. This hypothesis allows the ephebes to imagine as witnesses familiar and concrete entities ('boundary-stones,' which, as we have seen above, could assert both their presence and a truth claim by 'speaking' in the first person) rather than an abstraction ('borders') reified as a set of agricultural products. Moreover, given its general 'watching-over and guarding' function (see above), the *horos* qua boundary-stone would seem to make an eminently suitable witness to an oath. And yet the

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[30] A paratactic reading is assumed by P. Vidal-Naquet (1986) *The Black Hunter: Forms of Thought and Forms of Society in the Greek World.* trans. A. Szegedy-Maszak (Baltimore: 1986) 122: the ephebe guarantees 'to protect the boundary-stones of his country, and with them, the cultivated fields.' A metonymic reading seems to be assumed by Siewart (1977) 109. The latter reading might be supported by Thucydides' reference (6.13.1) to the sea as the *horos* between Athens and Sicily.

[31] For Alcibiades' reputed skill as a speaker: Plutarch, *Alcibiades* 10.2–3, 13.1–2; as a sophistic interlocutor: Xenophon, *Memorabilia* 1.2.40–46.

metaphorical reading raises its own problems: just as the *horos tês agoras* formally delimited the physical space of the agora, the 'boundary-markers of the *patris*' should delimit the physical fatherland, the polis itself. Thus *horoi tês patridos* can hardly refer to each and every field boundary in Attica.

In order to try to solve the conundrum, we will have to look outside the text of the oath itself, and consider some aspects of its context. In Athenian myth and fourth-century military practice, the ephebes were associated with and responsible for patrolling the frontier zones of Attica.[32] Assuming that this association of ephebes with the frontier extended back to the time of the archaic oath, we might narrow the metaphoric reading suggested above: the referent implied by *horoi tês patridos* was, I would suggest, the line of field *horoi* which defined the outermost reaches of Athenian agricultural enterprise. Within this line of *horoi* was the fatherland. The territory outside the line was in some sense wilderness: undesignated, undefined, unmarked.

Once again, we are confronted with a complete absence of archaeological evidence that might clarify our literary testimonia. No inscribed Athenian *horos* 'of the *patris*' has ever been found.[33] The text of the oath itself may suggest an explanation for their

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[32] Ephebes and frontiers: in myth: Vidal-Naquet (1986); in practice: Ober (1985) 90–95. Siewart (1972) 109, claims that the lack of an explicit reference to frontier zontes (*eschatiai*) is evidence for the ephebic oath's archaic character.

[33] Markers of national and even ethnic boundaries are mentioned by ancient authors, e.g. Plutarch, *Theseus* 25.3 (the stele which Theseus was supposed to have established at the Isthmus, stating on one side 'Here is Ionia, not Peloponnesus,' on the other, 'Here is Peloponnesus, not Ionia'); Xenophon, *Anabasis* 7.5.13 (Thracian tribes which establish piracy zones with στήλαι ὁρισάμενοι). Lycurgus 1.113, 115, could speak of the bones of the Athenian Phrynichus being dug up and 'thrown beyond the borders of Attica' (ἐξορίσαι ἔξω τῆς Ἀττικῆς); cf. Dinarchus 1.77. Pausanias mentions frontier markers of various sorts: 2.38.7; 8.25.1, 34.6, 35.2, 38.7. Moreover, some 150–200 inscriptions dealing with various aspects of polis borders (including border-markers from e.g. Aitolia, Laconia/Messenia, and Boiotia) have been found; see e.g. *SIG*[3] 933; *SEG* 23.297, 35.406; *IG* V.1 1371-1372, VII 2792, IX.1 427, IX.1[2] 1, 116. See P. Roesch, (1965) *Thespies et la confederation beotienne* (Paris: 1965) 61–63; M.K. Langdon, (1985) 'The Territorial Basis of the Attic Demes,' *Symbolae Osloenses* 60 (1985) 13–14 n. 4; M. Sartre, (1979) 'Aspects economiques et aspects religieux de la frontiere dans les cites grecques,' *Ktema* 4 (1979), 216–217; Daverio Rocchi (1988), 93–177; J.M. Camp II, (1991) 'Notes on the Towers and Borders of Classical Boiotia,' *American Journal of Archaeology* 95 (1991) 195 n. 9; D. Rousset and P.P. Katzouros, (1992) 'Une delimitation de frontiere en Phocide,' *Bulletin de Correspondence Hellenique* 116 (1992) 197–215. A corpus of border inscriptions has been announced by Rousset.

invisibility. The ephebes taking the oath promise not only to defend their homeland, but also to augment and extend it: 'I shall not hand down a lessened *patris*, but one that is increased in size and strength.' If the young men swearing the oath took that statement literally (and supposed that the goddesses Auxo and Thallo took an interest in the growth and increase of the *patris* proper, as well as of crops and livestock) then they could not in good conscience allow the *horoi tês patridos* to remain fixed. It would be their sacred duty to increase the extent of the cultivated land, by pushing back the boundaries of the wilderness.[34] As this process continued and new fields were marked out, the outer line of the *horoi* was necessarily and continually constructed anew. And thus, the precise referent of the phrase *horoi tês patridos* was inherently unstable—and its semiotic instability was a direct function of the sincerity with which the speech act of the oath was performed.[35] If we imagine the *horoi* in question to be unworked stones designating the borders of agricultural fields, similar to the Homeric example, the exact stones in question would not be the same from generation to generation: an individual *horos* which once designated the boundary of the *patris* would, by the fulfillment of the terms of the oath, become an 'internal' *horos*; the distinction it marked would consequently be altered significantly and it would no longer be included in the witnesses for the oath sworn by the next generation of ephebes.

The ephebic oath might be read as a textbook example of a self-deconstructing text: the deathless 'witnesses' of the oath, the guarantors, intended as fixed and stable points of reference, are themselves destabilized (their meanings radically altered) by the fulfillment of the conditions imposed upon the oath-takers. The demand for augmentation in the oath renders impossible the stability of the witnesses who are supposed to ensure that the demand is fulfilled. The changeless referents

[34] On the 'internal colonization' of Attica and the expansion of arable agriculture into marginal land on the frontiers, see R. Osborne (1985) *Demos: The Discovery of Classical Attika* (Cambridge: 1985) 6–10; T.W. Gallant (1982) 'Agricultural Systems, Land Tenure, and the Reforms of Solon', *Annual of the British School at Athens* 77 (1982) 111–24.

[35] The oath, as a form of promise, is a standard form of speech act: Austin (1975) 157–60 ('commisives'); Searle (1969) 54–62: 'as illocutionary acts go, [promising] is fairly formal and well articulated; like a mountainous terrain, it exhibits its geographical features starkly' (54). Note that the instability still pertains even if we accept the metonymic hypothesis and accept the agricultural products as the *horoi* in question.

of the oath and the established distinctions they mark cannot survive in the endlessly changing world which the oath itself enjoins. The inherent instability of the text and its witnesses may in turn have encouraged new (and sometimes seemingly perverse) citations and interpretations: Alcibiades is able to cite the oath as authorizing a reckless and (when viewed in retrospect) destructive plan for overseas imperialism. Even if we regard Alcibiades' sophism as a special case, however, there is surely no doubt that the ephebes who swore the oath in the post-imperial era of the later fourth century, when the oath itself was reinscribed, had something quite different in mind than their archaic and mid-to-late fifth-century predecessors: The wording of the oath might be *patrios* ('ancestral,' i.e. a word-perfect citation of the archaic original), but its meaning was not.[36]

Does the claim that the referent of the term *horoi tês patridos* was unstable and that the meaning of the oath itself changed over time, necessarily lead to a 'strong' deconstructive conclusion: i.e. that this text is evidence for the inevitable and general collapse (or even impossibility) of meaning itself? I would say not. At the solemn annual oath-taking ceremony each ephebe performed a speech act (more or less sincerely), and that act was felicitous in that it was comprehensible and regarded as binding by both the oath-taker and his community. The fact that a historian can show that the meaning of the oath and the identity of some of its witnesses must have changed over time need not lead us into the functionally useless assumption that words, meanings, signs, and referents are somehow unreal for the members of any given human society at any given point in its history.[37] Each generation of ephebes supposed it knew what the oath entailed and who (or what) were its witnesses. The ephebes either kept their promise or failed to do so, and were judged accordingly by their community. It is the social contingency of meaning, rather than its ontological indeterminacy, that is historically significant. Accepting social meanings as contingent (as opposed to utterly indeterminate) does not make historical explanation impossible. But it does

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[36] The fourth-century copy of the oath is prefaced by the statement Ὅρκος ἐφήβων πάτριος, ὃν ὀμνύναι δεῖ τούς ἐφήβους: 'the ancestral oath of the ephebes, which the ephebes must swear' (line 5). The 'post-imperial' fourth century: see below.

[37] What I mean by useless: J. Ober (1989) 'Models and Paradigms in Ancient History,' *Ancient History Bulletin* 3 (1989) 134–37.

entail the closest possible attention to details of social context. This consideration underlines the vital importance of incorporating both 'historical' and 'archaeological' evidence, both texts and artifacts, when we attempt to describe and to explain what was going on in past cultures.[38]

BORDERLANDS: *TA METHORIA*

I suggested above that the land which lay outside the *horoi tês patridos* could be regarded as 'wilderness: undesignated, undefined, unmarked.' Yet the land beyond the established frontier was not a trackless wilderness in any simple sense; rather it was a border zone that led eventually to the established frontiers of Megara or the poleis of Boiotia. The borderland between poleis was sometimes referred to as *gê methoria* (Thuc. 2.27.2, 4.56.2) or *ta methoria* (Thuc. 2.18, 5.3.5): literally that which lay 'between the *horoi*.'[39] Athenian actions which fulfilled the expansivist goals of the ephebic oath tended to bring land once outside the borders within the full political control of the state. Because this process was incremental, *ta methoria* remained a rather fuzzy category: yesterday's borderland was today's Attic land. The Athenian garrison town of Oinoe provides a good example. Oinoe was described by Thucydides (2.18.1–2) both as 'in Attica' (τῆς Ἀττικῆς) as 'in the *methoria* between Attica and Boiotia' (ἐν μεθορίοις τῆς Ἀττικῆς καὶ Βοιωτίας). The chronological reference point of Thucydides' comment is 431 B.C., the first year of the Peloponnesian War. And yet by that time Oinoe certainly almost enjoyed full deme status and so was a constituent part of the Athenian polis.[40]

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[38] For a thoughtful and detailed discussion of this issue, see I. Morris (1992) *Death-ritual and Social Structure in Classical Antiquity* (Cambridge: 1992).

[39] Cf. Thuc. 1.103.4: Megara allied with Athens because Corinth was attacking the former in a war about border land (περὶ γῆς ὅρων); 1.122.2: the Corinthians remind the Spartans that the coming conflict with Athens will be much different from disputes about border land (περὶ γῆς ὅρων αἱ διαφοραί). On the dynamism of internal and external frontiers and on border zones vs. border lines, see Daverio Rocchi (1988), 20–47.

[40] Thuc. 2.18.2. Status of Oinoe: D. Whitehead (1986) *The Demes of Africa, 508/7 ca. 250 B.C.* (Princeton: 1986) 48, 373, 403. Description of the site: Ober (1985) 154–155, 224; H. Lauter (1992) 'Some Remarks on Fortified Settlements in the Attic Countryside,' in *Fortificationes Antiquae. McGill University Monographs in Classical Archaeology and History* 12, eds. S. Van de Maele and J.M. Fossey (Amsterdam: 1992) 77–91. Note that there are two demes named Oinoe in Attica; I refer here to Hippothontid Oinoe, the western of the two. Herodotus (5.74.2) describes both Oinoe and Hysiai

We cannot say for sure how long before 431 the town of Oinoe was founded or when the land it occupied became part of Attica. The very probable archaeological site of Oinoe (Myoupolis in northwestern Attica) has not been excavated, but a good number of surface sherds have been collected; notably none of these has (so far) been dated earlier than the mid-fifth century. It is a reasonable guess (although, pending excavation, no more than that) that the town was founded by Athenians sometime in the late archaic or early classical period.[41] Whenever the town was incorporated, however, the Thucydides passage shows that in the latter fifth century Oinoe could still be described as being located 'between the *horoi*.' And thus, the term *ta methoria* could be used (even by a self-consciously precise author like Thucydides) conventionally as well as referentially. It proves to be no easier to define with precision what it meant for a classical writer to say that some place was 'between the *horoi*' than it is to fix the meaning of the *horos* itself.

The extension outwards of the borders of Attica and Athenian encroachment on the *methoria* were not processes that could be indefinitely prolonged without provoking Athens' neighbors. The wild, uncultivated *methoria* may seem in one sense a structural inverse of the cultured world of the polis, and thus an ideal liminal space for the conduct of rîtes de passage such as the *ephêbeia*.[42] But in the world of inter-polis diplomacy, which depended on establishing distinctions that would be recognized and respected by discrete political entities, it was necessary that the *methoria* be defined in functional terms as carefully as was the land within the borders. As in the case of the *horos*-delimited land we have considered above,

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(which was never a deme in the Athenian constitutional sense) as δήμους τοὺς ἐσχάτους τῆς Ἀττικῆς ('frontier towns of Attica').

[41] Date of pottery from the probable deme site: J. Ober (1987) 'Pottery and Miscellaneous Artifacts from Fortified Sites in Northern and Western Attica,' *Hesperia* 56 (1987) 211–12; C. Edmonson (1966) 'The Topography of Northwest Attica,' (Unpublished Ph.D. dissertation, University of California at Berkeley 1966) 32. H. Lohmann (1992) 'Agriculture and Country Life in Classical Attica', in *Agriculture in Ancient Greece. Acta Instituti Atheniensis Regni Sueciae* 42, ed. Berit Wells (1992) 35, suggests that some demes (including Atene and Piraeus) may not yet have been founded at the time (in 508/7 or 507/6) Kleisthenes established the political system based on demes, trittyes, and tribes.

[42] Thus Vidal-Naquet (1986) 106–28. On mountainous borderlands in Greek myth as areas that are 'outside and wild' see R.G.A. Buxton (1992) 'Imaginary Greek Mountains,' *Journal of Hellenic Studies* 112 (1992) 7–8.

the permitted uses of the *methoria*—who had a right to do what in that space, and under what conditions—had to be spelled out. Such was the situation in the region of Panakton, a site in the central Athenian-Boiotian *methoria.*[43] By the time of the Peloponnesian War, the Athenians had built a fortress there; the place was captured by the Boiotians early in the war, and they proceeded to slight the walls before handing it back to the Athenians under the terms of the Peace of Nicias. When the Athenians protested the slighting, the Boiotians produced the following excuse (*prophasis*): 'as the result of a dispute, there were ancient oaths in force to the effect that neither side was to possess the place, but they were to graze it in common.'[44]

It is worth noting that the terms of the agreement cited by the Boiotians stipulate grazing, rather than arable agriculture as the permitted (common) use of the land in question. But regulation under a formal interstate agreement is far from the romantic image of the frontier zone as an unrestricted and undefined wild space. On the other hand, the Athenian action in building the fort, which (at least according to the Boiotians) contravened the old agreement, demonstrates how very difficult it might be to maintain agreed-upon inter-border land-use standards when more than one society was involved. In this case the expansivist ideals we have seen embedded in the ephebic oath (*horkos ephêbôn*) evidently overrode the 'ancient oaths' (*horkoi palaioi*) sworn by the Athenian ambassadors to their Boiotian counterparts; the speech-act conventions pertaining within Athenian society proved stronger than those employed between the two states. The Boiotian attempt to 'annul' the inter-polis boundary-marker represented by the Athenian fortress at Panakton proved to be unsuccessful in the long run. The fort was rebuilt by the Athenians in the fourth century and was indeed used as a base for Athenian ephebes.[45]

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[43] In *methoria*: Thuc. 5.3.5. The site of Panakton: Ober (1985) 152–54; Ober (1987) 209–11; M.H. Munn (1990) 'On the Frontiers of Attica and Boiotia: The Results of the Stanford Skourta Plain Project', in *Essays on the Topography, History, and Culture of Boiotia. Teiresias Supplement* 3, A. ed. A. Schachter (Montreal: 1990) 32–40.

[44] Thuc. 5.42.1: ἐπὶ προφάσει ὡς ἦσάν ποτε Ἀθηναίοις καὶ Βοιωτοῖς ἐκ διαφορᾶς περὶ αὐτοῦ ὅρκοι παλαιοὶ μηδετέρους οἰκεῖν τὸ χωρίον, ἀλλὰ κοινῇ νέμειν.

[45] This has been conclusively demonstrated by a Stanford University survey and excavation; the field work has not yet been fully published but cf.

The Athenians' tendency to augment their own polis militated against the very concept of *gê methoria* as frontier zone and may, ironically, have given impetus to the eventual establishment of relatively stable linear frontiers. I have argued elsewhere that the period after the Peloponnesian War saw a systemmatic attempt on the part of the Athenians to rationalize their northern and western land frontiers through the construction of a system of fortresses and watch-and-signal towers. This program represented a significant change from former practice: rather than establishing new fields or garrison towns in order to roll back the borders, the Athenians now focused their attention on the attempt to define a stable border line.[46] Although the ephebes continued to swear that they would augment the size and strength of the fatherland, that promise would no longer be fulfilled by extending Athenian control over the *methoria*. The line of border fortifications implied the existence of a fixed linear frontier, or at least signalled an Athenian desire that stability should pertain. Although even in the fourth century we have no epigraphic examples of Athenian state-border *horoi*, the forts and towers themselves may be thought of as representing a line of border-markers which defined the limits of the Athenian *patris*.[47]

RUPESTRAL *HOROI* OF ATTICA

What effect might the stabilization of the national frontier have had on the production of social meanings assigned to intrapolis *horoi*? This question brings us back to a consideration of

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summary in M. Munn, and M.L. Zimmerman Munn (1989) 'The Skourta Plain Project: the 1987 and 1988 Seasons of Survey on the Attic-Boiotian Frontier,' *American Journal of Archaeology* 93 (1989) 274–75; and Munn (1990).

[46] Ober (1985) esp. 208–220. This should not be taken as a hard and fast rule: cf. disputes with Thebes over the border zone of Oropos and with Megara over the sacred Orgas: Ober (1985) 214–17.

[47] Even if we do not accept agricultural products as *horoi*, there is no reason to suppose that a border-marker need always take the form of a stele. Herms and other artifacts were evidently commonly used as border markers: L. Kahn (1979) 'Hermès, la frontière et l'identité ambiguë', *Ktêma* 4 (1979) 201–11; Sartre (1979); Daverio Rocchi 1988, 53–59. [Demosthenes] 7.40, for example, claims that the border of the Chersonese peninsula was marked by an altar of Zeus 'of the borderland.' Lycurgus 1.143 claims that his opponent will beg the jurors to be allowed to live 'within the walls of the *patris* (ἐν τοῖς τείχεσι τῆς πατρίδος); given the associations of the term *patris* discussed above, this might be taken to mean 'within the territory protected and defined by the fortifications of the fatherland' (rather than, or as well as, 'within the walls of the city proper').

archaeological evidence proper. In the course of the last decade, a fair number of ancient rupestral (rock-cut) *horoi* have been published from various sites in southern Attica. Although the inscription sometimes includes an indication (severely abbreviated and thus cryptic) of what sort of territory is being delimited, often just the four letters of the word itself (ΟΡΟΣ) were cut directly into an exposed shelf of bedrock. In several cases, the rupestral *horos* is clearly part of a series which defines a line or even a corner.[48] The single-word rock-cut *horos* is, at first glance, the simplest possible sort of text and simultaneously the result of a very basic process of artifact production: the natural bedrock is turned to human uses through the action of inscribing the four letters. The rupestral *horos* can be viewed as a particularly pure example of text-as-artifact and artifact-as-text: viewed as a text the *horos* is nothing but an artifact (a word in lieu of a stele); viewed as an artifact it is nothing but a text (the bedrock is unaltered except for the inscribed letters).

The physical presence of these simple inscriptions on the bare rock of Attica brings us face to face with the *horos'* basic function: those who chiseled the letters into the bedrock clearly intended to establish a distinction or set of distinctions. Before the letters were cut, the rock face was a simple, brute fact of nature. With the removal of a few chips of stone, that selfsame fact of nature was redefined in overtly cultural, social terms. The rupestral inscription is, by definition, *in situ*; indeed its precise situation and immovability seem intrinsically bound up in its meaning. Unlike Athena's stone, the rupestral *horos* (which exists as an inscribed absence of material, rather than a material presence) cannot be picked up or pushed aside. Unless it is chiseled away or covered up (by human agency or a natural accumulation of debris) it both keeps its place and maintains its claim to establish distinction in this particular place. When several *horoi* form a line, we can say with certainty that the social meaning once imposed upon the ground lying on one side of the line was non-identical to that imposed upon the ground on the other side: As long as there are humans able to construe the four inscribed signs as letters spelling out a word for 'border-marker,' the rock-cut *horoi* will continue to proclaim, 'Over here, "this"; over there, "that"'

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[48] Series of *horoi*: List in Traill (1986) 117. For other examples of Attic rupestral *horoi* (individual and series), see below, note 58.

But, perhaps to an even greater extent than in the cases of *horoi* attested in literary sources, the rupestral *horos* remains silent in regard to both the nature of the significance it marked and the agency which authorized the establishment of a distinction. In the absence of an explanatory context, we can say nothing with certainty about a isolated single-word rock-cut *horos* beyond the claim that a distinction was once established at the point of its situation. Nor does the existence of a line of *horoi* get us much further, except insofar as we can now plot the distinction spatially. Chronological context turns out to be very hard to come by: Dating rupestral *horoi* can be a hazardous enterprise; close dating is usually impossible. Exposed bedrock has no stratigraphy; surface sherds (when they happen to be found) cannot be securely associated with the date of the inscription's cutting. We lack a dated typological sequence for rock-cut letter forms; and the relationship between the relatively huge, roughly-cut letters of the rupestral *horos* and the careful work of professional stone-cutters on non-rupestral inscriptions remains somewhat tenuous.[49]

Yet there is one notable point about Athenian rupestral *horoi* that has been taken by some scholars as pointing to a context: their occurrence is quite widespread within Attica. Several series of one-word *horoi*, all of which resemble one another in that they employ lunate (rather than three- or four-bar) sigmas, have been found on hills in southern Attica: on Mt Lykabettos, on Alepovouni in the southwestern foothills of Mt Hymettos, on Kaminia in southeastern Attica near Vari, on Megalo Baphi near Legrena, and on a ridge east of Mt Merenda.[50] It is cer-

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[49] Traill (1986) 118 states that 'the date of the horoi is problematical, but several have been assigned, on good authority, to the 4th century B.C., and I believe all may be dated to that same period'; the 'authority' in question is not identified. My own suggestion (J. Ober (1981) 'Rock-Cut Inscriptions from Mt. Hymettos', *Hesperia* 50 (1981) 76) that the four (lunate sigma) Alepovouni *horoi* date to the Roman era on basis of letter forms was accepted by Langdon (1985a) 257–58 and Whitehead (1986) 29 n. 110, but rejected by Stanton (1984) ('Some Attic Inscriptions,' *Annual of the British School at Athens* 79 (1984) 301–03) and Traill (1986) 119. For attempts to date other rupestral *horoi*, see e.g. J. Traill (1982) 'An Interpretation of Six Rock-cut Inscriptions in the Attic Demes of Lamptrai,' in *Studies in Attic Epigraphy, History and Topography Presented to Eugene Vanderpool. Hesperia Supplement* 19 (Princeton: 1982) 168; Langdon (1985b) 'Hymettiana I', *Hesperia* 54 (1985) 257, 260; M.K. Langdon (1988a) 'The ΖΩ/ΒΑ *Horoi* at Vari in Attica,' *Greek Roman and Byzantine Studies* 29 (1988) 50; M.K. Langdon (1988b) 'The Topography of Coastal Erechtheis,' *Chiron* 18 (1988) 77–78.

[50] These are tabulated and discussed in Traill (1986) 116–22. Traill #4 =

tainly possible to argue (if not to prove) that these various inscription series were cut at about the same time. Assuming that to be the case, one may be willing to take the next (rather large) step and suppose that the inscriptions were cut at the direction of a single authorizing agency. And finally, given their scattered proveniences and the rather large areas they seem to define, one who has swallowed the two previous premises might further speculate that the authority in question was the central government of the polis. And it would then seem reasonable to ask what the Athenian government's purpose might have been in establishing boundaries at various places around Attica. This chain of reasoning brings us back (albeit in a rather roundabout way) to the question posed above, regarding the impact of fixed borders upon the internal subdivision of Attica. If the chain is sound and if the inscriptions could be dated to the century after the Peloponnesian War, then we might be in a position to make some interesting suggestions about fourth-century Athenian attitudes towards borders generally. But if any one of the chain's links is very weak, we will have to admit that the rupestral *horoi* will not help us solve this historical problem.

A number of scholars have in fact asserted that one or more of several south Attic rupestral *horos* series (including the various lunate-sigma series noted above) were cut in the fourth-century B.C. at the behest of the Athenian state, in order to regularize or reorganize the borders between Attic demes.[51] This is an extremely seductive notion in that it transforms the simple *horos* into highly significant evidence for an important political program. It means that historians can employ spatially fixed

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two on Lykabettos (ΟΡΟϹ with lunate sigma; one now lost): *Inscriptiones Graecae* II^2 2521; Traill (1986) 117, 119–20. Traill #6 = two (ΟΡΟϹ with lunate sigma) on the ridge of Kaminia near Vari in SW Attica: Lauter (1982); Langdon (1988b). Traill #5 = two (ΟΡΟϹ with lunate sigma) on a ridge and saddle E of Mt Merenda: Traill (1986), 117, 120. Traill #2 = five (perhaps more: ΟΡΟϹ with lunate sigma) in line along the ridge of Megalo Baphi near Legrena in south Attica: H. Lohmann (1983) 'Atene ('Ατήνη) eine attische Landgemeinde klassischer Zeit', *Hellenika: Jahrbuch für die freunde Griechenlands* (1983) 99–104; Lohmann (1992) 33. Traill #3 = four (ΟΡΟϹ with lunate sigma) on Alepovouni, a western foothill of Hymettos, defining a line and corner: Ober (1981) 73–77. Traill #1 = six (ΟΡ(ΟΣ/ΠΜ) on the ridge of Panagia Thiti (139 m) southeast of Hymettos: Traill (1982).

[51] Traill (1982); Lauter (1982); Stanton (1984); Whitehead (1986), 29 n. 110 (doubting that the Panagia Thiti and Alopovouni series were deme-markers, but accepting the Megalo Baphi series); Langdon (1988b); Lohmann (1992) 33.

archaeological evidence to define the precise borders of demes and hence estimate the size, resources, even populations of individual demes. The deme-marker hypothesis turns these otherwise maddeningly laconic markers into evidence that might be applied to a variety of historical puzzles, from Kleisthenes' original plans to (per above) the impact of state border stabilization on the political organization of the polis. On the whole, it would be very good news for historians of classical Athens if the deme-boundary explanation for the rupestral *horoi* were correct.

Is it correct? In 1981, before most of the *horos*-series from other parts of Attica had been reported, I published a series of four rupestral *horoi* from Alepovouni on the slopes of Mt. Hymettos. I suggested that the Alepovouni *horoi*, which describe a corner (three inscriptions in line with a fourth at a right angle to the third) were best explained as marking the corner of a Roman bee-farm.[52] That explanation was challenged in separate studies by H. Lauter (1982, 315) and G.R. Stanton (1984, 301–303); the latter argued that the letter forms were less diagnostic than I had supposed and might be much earlier. Lauter and Stanton advanced the classical deme-marker explanation for the four Alepovouni *horoi* and for a *horos* on Kaminia. J.S. Traill (1982) meanwhile suggested that a series of *horoi* (reading ΟΡ/ΠΜ, rather than ΟΡΟϚ) on the Panagia Thiti ridge south of Hymettos were late fourth-century deme-markers.[53] Shortly thereafter, H. Lohmann (1983) adopted the classical deme-marker explanation for the recently discovered Megalo Baphi series. M. Langdon (1985a) in turn rejected the Lauter/Stanton deme boundary hypothesis for the Alepovouni *horoi*, partly on the grounds that deme boundaries should follow the natural contours of the ground and should not therefore make right-angle turns. Langdon, following an earlier suggestion of G. Culley, proposed associating the Alepovouni *horoi* with a Roman-era inscription which mentioned 'public highlands' (*orê dêmosia*) in the Hymettos region and concluded that 'the temptation to link this information to the *horoi* on [Alepovouni] is practically irresistible.'[54] By 1986 Traill (1986:

[52] Ober (1981) 73–77.

[53] The series was originally published by C.W.J. Eliot (1962) *Coastal Demes of Attika: A Study in the Policy of Kleisthenes. Phoenix Supplement* 5 (Toronto: 1962) 63–64.

[54] Langdon (1985a) 259. The inscription is *Inscriptiones Graecae* II^2 1035 (line 58); the Alepovouni *horoi* are associated with the Roman inscription

116) was ready to assert that 'the demes were served by rupestral *horoi*' and felt it no longer necessary even to argue the case that most of the known rupestral *horoi* of Attica (and several non-rupestral examples) marked deme boundaries. Langdon (1988), who found a second *horos* on Kaminia, was subsequently persuaded by Traill to adopt the fourth-century deme-marker explanation for the two Kaminia *horoi*. Yet accepting the explanation Langdon found 'practically irresistible' for the Alepovouni series and his preferred explanation for the Kaminia inscriptions entails separating the Kaminia series from the Alepovouni series in date and function, and so the argument from similarity of appearance must be discarded. On the other hand, Traill's interpretation of the Panagia series, with its different text, raises the possibility that variant types of *horoi* could have the same (deme-marker) significance.[55] And thus we are left with the possibility that similar texts may have different meanings; and dissimilar texts identical meanings.

My bee-farm hypothesis was, I admit, simply a speculation, based on the ancient reputation of Hymettos as a honey-producing region, the existing vegetation (especially thyme) in the area around the *horoi* themselves, and what I took to be their date. The deme-marker explanation is predicated in part on the existence of *horos*-series in different geographical locations around Attica. Does the multiplicity of superficially similar inscriptions move the latter explanation out of the realm of speculative hypothesis? Regrettably, it does not. First, it is not possible to prove that a single agency was responsible for all of the *horoi*: imitation (e.g. by private individuals or by local officials) would explain the physical evidence just as well as unitary authorship. Next, there is no necessary reason to suppose that all the lunate-sigma rupestral *horoi* were cut at or even about the same time. If we do not assume unitary state

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by G. Culley (1977) 'The Restorations of Sanctuaries in Attica II,' *Hesperia* 46 (1977) 270 n. 27.

[55] Traill (1986) 116, suggests that '[T]he peculiarity of 1 [the ΟΡ/ΠΜ series] may be explained by the fact that this group segregated a special pair of demes which belonged to two different trittyes'; but the argument is circular, since the 'fact' in question is actually Traill's hypothesis, which is in turn based on the assumption that the inscriptions in question are indeed deme boundary markers. In support of his proposed reading of ὅρ(ος) π(αραλίας καὶ) μ(εσογαίας), Traill (1982) 168–69, cites a series of three inscriptions on Mt. Paximadi between ancient Laconia and Messenia: Μ/ΟΡΟ/Λ (=ὅρο(ς) Μ(εσσήνη καὶ) Λ(ακεδαίμονι); ΟΡΟ(Σ); ΟΡΟΣ ΛΑΚΠΡΜΕΣ (=ὅρος Λακ(εδαίμονι) πρ(ὸς) Μεσ(σήνην): *Inscriptiones*

authorship a priori, a standard diffusionary model would lead us to expect that formally similar rupestral *horoi* would appear considerably later at locales distant from the point of invention and first use.[56] Third, the form of even the several series of single-word lunate-sigma inscriptions is far from standardized: the notable differences in size and lettering style seem to preclude the possibility that a single lettering crew was at work.[57]

In sum, any given *horos* series certainly might have defined a deme boundary at some point in its history, but equally possibly, it might not have. There exists, at the moment, no means of falsifying counterhypotheses (e.g. that the inscriptions are the traces of a fad for permanently marking the boundaries of private holdings or sacred precincts that began in the immediate environs of the city and eventually spread to the south and east), and no way of refining the chronology.[58] Nor can the Gordian knot be cut by assuming that all Attic rupestral *horoi*, regardless of form or text, are classical

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Graecae V,1 1371 a–c. This series might also be taken as proof that different texts could carry identical meanings; although other readings of the texts and their meanings are of course possible.

[56] Spatial diffusion and chronological seriation: J. Deetz and E. Dethlefsen (1965) 'The Doppler Effect and Archaeology: A Consideration of the Spatial Aspects of Seriation', *Southwestern Journal of Anthropology* 21 (1965) 196–206. Note that the distances involved need not be great. Deetz and Dethlefsen's test case of gravestone motif preference showed considerable artifact seriation among several towns located in Eastern Massachusetts in the period 1700–1829; the greatest distance involved was about 35 km (straight line: Lexington to Groton). Coincidentally, the Alepovouni horoi (nearest the city) are about 35 km (straight line) from those at Megalo Baphi (furthest from the city).

[57] Letter heights and inscription length (in meters): Megalo Baphi #2: 0.14–0.20 (h) x 1.01 (l), #5 0.15–0.19 (h) x 0.84 (l); Kaminia #2: 0.08–0.12 (h) x 0.42 (l); Alepovouni #1: 0.26–0.32 (h) x 1.10 (l), #2: 0.12–0.17 (h) x 0.60 (l); Lykabettos #1: 0.065–0.13 (h); E of Merenda: #1,2: 0.12–0.14 (h). Style: Megalo Baphi: stem of rho seems to extend above loop (although this may be a wear mark) and well below the line. Kaminia: Relatively large loop on rho which begins at loop but extends below the line. Alepovouni: large omicrons, rho with very small loop, stem of rho strictly aligned with other letter bottoms. Lykabettos: very large, loopy rho, stem aligned with letter bottoms; Merenda: Large omicrons, rhos with fairly small loops, stems extend below the line.

[58] The distribution pattern of known rupestral *horoi* (map: Traill (1986) 121, from Aleopvouni to Megalo Baphi, could be accommodated by the simplest sort of diffusion model (assuming the city of Athens as the point of origination). The argument of Lohmann (1992) 33, that since the Megalo Baphi inscriptions extend across the heads of two valleys, they must be deme markers, is far from definitive, since there is no way to preclude a very large temenos or private holding. Sacred precincts defined by *horoi*:

deme-markers; there are simply too many *horoi* that must be left out of any single, comprehensive explanation of their meaning.[59] Since some rupestral *horoi* clearly are not deme-markers, the argument becomes completely subjective: *horoi* are deme-markers when they show up in places where one supposes deme-markers should show up; other explanations (or no explanation) can be adduced when they show up where one does not want deme-markers showing up. The level of subjectivity would be considerably lessened if there were independent textual authority pointing clearly to where we should look for deme-borders, but no such evidence has as yet been brought forward.[60]

The argument for accepting the several series of *horoi* discussed above as classical deme-markers (or as markers of a Roman bee preserve) requires reading certain very specific sorts of signification onto ambiguous marks, while denying the possibility (or at least the likelihood) of all other possible significations. In the case of the deme-marker hypothesis, the driving force behind the exclusionary reading is the intense longing of historians and archaeologists alike to be able to delimit specific demes at specific times in history. Yet in the end, the argument rests precariously on the linked a priori assumptions that if a deme was a distinct geographical entity with secure borders, then its borders should be defined by

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above n. 9. Traill (1986) 119, rather uncomfortably allows for the *temenos* hypothesis, but only as subsidiary to the deme-marker hypothesis, in the case of the Alepovouni series: 'I will not reject the possibility that a hill-top sanctuary shared by two demes . . . might be included within their [the inscriptions'] compass.'

[59] E.g. four inscriptions reading HO, near the church of Panagia at Thiti: Eliot (1962) 56–58; Traill (1982) 168 n. 22 (both assume the inscriptions mark the peribolos of a sanctuary). An inscription on Alepovouni reading ΟΡΟΣ (four-bar sigma), near, but not in line with with the other four: Langdon (1985a) 257–60. Two others (not in series) on the western slopes of Hymettos: ΟΡΟΣ (*Inscriptiones Graecae* II[2] 2519), ΖΗΝΩΝΟϚ ΟΡΟϚ (*Inscriptiones Graecae* II[2] 2713). Three on hill 337.4 of Hymettos, reading ΟΡΟΣ ΔΙΑΝΟ(ΜΟΥ) (=boundary of the water conduit): Langdon, (1985a) 260–62. Three on the hill of Lathoureza near Vari, reading ΖΩ/ΟΡΟ(Σ)/ΒΑ: Langdon (1988a).

[60] Langdon (1985a) 7, explores the literary testimonia, and concludes that 'our search does not yield much evidence.' The most important is *Ath. Pol.* 21.4: διένειμε [sc. Kleisthenes] δὲ καὶ τὴν χώραν κατὰ δήμους τριάκοντα μέρη ('he divided up the territory by demes into thirty parts'). But this is pretty vague. Cf. Lohmann (1992) 33, who baldly asserts that 'since literary sources testify to such deme boundaries in Attica, there should no longer be any doubt as to their existence.' But he does not cite the sources he has in mind.

prominent ridges separating agricultural valleys, and so if *horoi* are found on an appropriate ridge, then they must be polis-established boundary markers.[61] Lacking secure textual evidence for demes as geographical entities or for the nature of their boundaries, the deme-marker hypothesis remains in the category of speculation. The iterability of the marks in question and the potential for the imposition of new meanings on existing marks still defeats the hope of securely fixing their significance. Although the rupestral *horoi* are fixed in their location, and so cannot be grabbed up by a god looking for a weapon or pulled up by order of a political reformer, in the long run their meanings were no more securely fixed than those of Athena's stone, the pre-Solonian *horoi*, or the *horoi* of the *patris*.

The ancient rupestral *horoi* are still too spare; there is as yet no way to provide them with a context secure enough to control the range of meanings they might once have proclaimed. Historians and archaeologists must not allow either the desirability of the most interesting explanation of archaeological evidence (which is certainly the deme-marker hypothesis) or bare assertions by scholars (however distinguished) to stand for proof of a hypothesis' correctness.[62] This rather depressing negative conclusion points to what seems to me an important (although also negative) archaeological point: a text that is nothing other than an artifact, an artifact that is nothing other than a text has remarkably little to say. Artifacts and texts alike gain meaning

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[61] The geographical argument that deme (and trittys) boundaries must exist and are defined by natural features, especially ridges: Eliot (1962) 136–38; Langdon (1985b); Traill (1986) 116–22. But cf. W.E. Thompson (1971) 'The Deme in Kleisthenes' Reforms', *SO* 46 (1971) 72–79; Whitehead (1986) 27–30. M.H. Hansen (1991) *The Athenian Democracy in the Age of Demosthenes: Structure, Principles and Ideology* (Oxford: 1991) 58, suggests that for ancient Greeks the population of citizens was more important than territory. Deme-officials known as *horistai* are mentioned in *Inscriptiones Graecae* II^2 1177, lines 21–24, but (as Whitehead (1986) 141, notes) we have no idea what their duties might have been.

[62] This does not mean that one should not propose speculative hypotheses; indeed speculation is a necessary and salutary part of the interpretive process. We must have working hypotheses, so that when further evidence is discovered (and I do not suppose that all the visible rupestral *horoi* of Attica have yet been found or published) those hypotheses can be strengthened or tossed aside. My point is that we should recognize these hypotheses for what they are. We must not suspend critical judgment in the face of an argument from authority (and a fortiori from anonymous authority). And we must guard against drifting into accepting a hypothesis as a 'foundational fact,' or discarding alternative hypotheses, simply because we *want* it to be true.

through their situation in a broader social and ritual context—when deprived of their context, they are also deprived of their power to impart meaning. The *horos* was intended by those who inscribed it as a speech act: an imperative to the members of society, ordering them to act in conformity with the distinction proclaimed by the marker. But outside its social milieu, that speech act is infelicitous. Shorn of context, the text-as-artifact/artifact-as-text can refer to nothing outside of its proclamation of self-presence and difference.

HISTORICAL TEXT AND ARCHAEOLOGICAL CONTEXT IN ROMAN NORTH AFRICA: THE ALBERTINI TABLETS AND THE KASSERINE SURVEY

R. BRUCE HITCHNER

INTRODUCTION

Justifiable emphasis has been placed in recent years on the importance of context in the interpretation of archaeological data.[1] Context is defined by Hodder as 'the totality of the relevant environment where 'relevant' refers to a significant relationship to the object—that is, a relationship necessary for discerning the object's meaning'.[2] Context can also depend, again following Hodder, on the nature of the questions posed.

Context is likewise critical to the understanding of and questions put to the meaning of past events or actions recorded in texts. In ancient history, where the textual record of historical events consistently falls short of capturing 'the totality of the relevant environment,' archaeology has often been called upon to fill the contextual lacuna.[3] There are, of course, difficulties in this approach having to do with determining the veracity of the historical event under scrutiny and the appropriateness of the archaeological evidence brought to bear.[4] The benefit, however, is that when these obstacles are overcome, a signal advance in the understanding of the event itself and of its relationship to larger and longer term patterns is frequently achieved.[5]

The employment of archaeological knowledge as a meaningful structure within which historically documented events can be located and interpreted has particular merit for the

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[1] I. Hodder (1991) *Reading the past*, 2nd ed. (Cambridge: 1991).

[2] Hodder (1991) 143.

[3] A. Snodgrass (1983) 'Archaeology', in *Sources for Ancient History*, ed. M. Crawford (Cambridge: 1983) 137–84, A. Snodgrass (1991) 'The Place and Role of the Annales School in the Approach to the Roman Rural Economy', in *The Annales School and Archaeology*, ed. J. Bintliff (Leicester: 1991) 77–103.

[4] A. Snodgrass (1987) *An Archaeology of Greece: the Present State and Future Scope of a Discipline* (Berkeley: 1987) 36–66; Hodder (1991) 128–46.

[5] Bintliff, ed. (1991).

study of Roman North Africa as much of our knowledge of the region's history under the empire is derived from private and official documents inscribed on stone, metal, or other mediums belonging to an extraordinarily fecund archaeological environment. That said, it must be admitted that the historiography on Roman North Africa is a record of lost opportunity in this regard, for despite the enormous explanatory potential latent in the archaeological heritage of the region, the tendency has been to privilege the epigraphic and literary evidence in the writing of the history of Roman Africa. As one scholar recently noted:

> The 60–70,000 Libyan, Punic, Greek, and Latin inscriptions have been the [his italics] crucial factor in the rewriting of the history of the African provinces. Almost every general work on the subject reflects this source, as opposed to archaeological evidence, as the basis of its historical reconstruction. Inasmuch as the epigraphy happens to exist in such abundance, it has been exploited in preference to any other type of evidence. This predilection has led to an imbalance in North African studies that would be hard to match in any other region of the empire.[6]

Nowhere is this lack of integration of the textual with the archaeological record more sorely felt than in the history of the North African countryside. Despite an archaeological archive that is unparalleled in most other parts of the empire, studies of rural economy and society in Africa in the Roman period have historically been written mainly on the basis of the literary and epigraphic record.[7] In recent years, the advent of archaeological survey has contributed a wealth of new information on the African countryside,[8] some of which has already begun to be integrated with the documentary evidence.[9] In this paper I

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[6] B.D. Shaw (1980) 'Archaeology and knowledge: the history of the African provinces of the Roman Empire', *Florilegium* 2 (1980) 32–33.

[7] The strength of this historiographical tradition is still evident, see, for example, D.P. Kehoe (1988) *The Economics of Agriculture on Roman Imperial Estates in North Africa* (Göttingen: 1988), a very valuable study of the agricultural economy of Roman Africa which depends almost entirely on epigraphic and comparative evidence.

[8] D.J. Mattingly (1992) 'The field survey; strategy, methodology, and preliminary results', in *Lepti Minus: Survey and excavation at a Roman port city, first preliminary report,* eds. H. Slim, J.H. Humphrey, N. Ben Lazreg, D.J. Mattingly (Ann Arbor: 1992) 24–47.

[9] P. Leveau (1984) *Caesarea de Maurétanie, une ville romaine d'Afrique et ses campagnes* (Paris: 1984); D.J. Mattingly 1988) 'Oil for Export? A Comparison of Libyan, Spanish and Tunisian Olive Oil Production in the Roman Empire', *Journal of Roman Archaeology* 1 (1988) 33–56; R.B. Hitchner and

wish to expand the horizons of this new integration by attempting to provide an archaeological context for one of the critical documents in the rural history of Roman Africa, the *Albertini Tablets.*[10] In so doing, I hope not only to advance our understanding of the complex events recorded on the tablets, but to further our knowledge of the agricultural economy of the African provinces. The context for this study is not, however, the poorly understood ancient landscape of the Djebel Mrata in southeastern Algeria (southern *Numidia*), the area where the tablets were discovered (Figs 1, 3).[11] Rather, I have selected in its place a Roman period landscape recorded by the Kasserine Survey which I directed during the 1980s in the Tunisian high steppe (western *Byzacena* in *Africa Proconsularis*) (Figs 1, 2).[12] Although a distance of some 75 kms separates

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D.J. Mattingly (1991) 'Fruits of Empire the production of olive oil in Roman Africa', *National Geographic Research and Exploration* 7.1 (1991) 36–55; B.D. Shaw (1991) 'The Noblest Monuments and the Smallest Things: Walls and Aqueducts in the Making of Roman Africa', in *Future Currents in Aqueduct Studies* ed. A.T. Hodge (Leeds: 1991) 63–91.

[10] The tablets were discovered in a cave somewhere in the Djebel Mrata in 1928 and brought to the attention of E. Albertini. The *Tablettes Albertini,* as they came to be known, were fully published by C. Courtois, L. Leschi, C. Perrat, and C. Saumagne (1952) *Tablettes Albertine. Actes Privés de l'epoque Vandale (fin du Ve siècle)* (Paris: 1952); and have been a subject of scholarly interest ever since. See, most notably, J. Lambert (1953) '"Les Tablettes Albertini"', *Revue Africaine* 97 (1953) 196–220; M. Pallasse (1955) '"Les Tablettes Albertini" interessent-elles le colonat romain du basempire', *Revue Historique de Droit Francais et Etranger* 4, 33 (1955) 267–81; J. Percival (1975) '*Culturae Mancianae*: field pattern in the Albertini Tablets', in *The Ancient Historian and his Materials. Essays in Hour of C.E. Stevens on his Seventieth Birthday,* ed. B. Levick (Oxford: 1975) 213–27; C.R. Whittaker (1978) 'Land and Labour in North Africa', *Klio* 60 (1978) 331–62; H. Pavis D'Escurac (1980) 'Irrigation et vie paysanne dans l'Afrique du Nord antique', *Ktema* 5 (1980) 177–91; D. Vera (1988) 'Terra e lavoro nell' Africa Romana', *Studia Storici* 4 (1988) 967–92; D.J. Mattingly (1989) 'Olive Cultivation and the Albertini Tablets', *L'Africa-Romana* 6 (1989) 403–15; P. Ørsted (forthcoming) 'From Henchir Mettich to the Tablettes Albertini', in *Landuse in the Roman Empire,* ed. J. Carlsen et al. (Rome: forthcoming).

[11] The specific location of the estate in the Djebel Mrata is unknown, but D.J. Mattingly (1989) 409, plausibly suggests that the estates were located along the Oued el-Horchane on the grounds that there is little evidence for olive presses in the vicinity of the Qued Tseldja near Moulares, the location favored by Courtois et al. (1952) 190–94, for the *fundus Tuletianos* and *fundus Capprarianus,* see fig. 3 (I wish to thank DJM for providing a copy of the map of the Djebel Mrata region). For the archaeological evidence in the region consult the maps in S. Gsell (1991) *Atlas Archéologique de l'Algerie, feuille* 51 (Paris: 1911) f.1 (Gafsa) and the report by C. Guenin (1909) 'Inventair archéologique du cercle de Tebessa', *Archives des Missions* 17 (1909) 98–101.

[12] R.B. Hitchner (1988) 'The Kasserine Archaeological Survey 1982–1986', *Antiquités africaines* 24 (1988) 7–41; R.B. Hitchner (1989) 'The organiza-

the survey area from the events recorded in the tablets, this potential problem in integrating the two bodies of data is, as I shall attempt to show, more than offset by the material, temporal and spatial similarities that exist between them, and the many questions regarding social and economic conditions on the estate that cannot be answered by the information provided in the tablets alone.

Archaeological Context: The Kasserine Survey

The survey was conducted in the vicinity of the Roman towns of *Cillium* (modern Kasserine) and *Thelepte* (Feriana) (Fig 1), and was designed to investigate the nature and extent of rural settlement and agricultural development in the semi-arid high steppe in the Roman period. The region of Kasserine was selected for study because of the large number of well-preserved Roman period sites scattered throughout the landscape, many with standing remnants of presses. Systematic field-walking was conducted by the survey over some 75 km^2 in five sectors (Fig 2). In each sector the survey identified many more sites and other archaeological features than were indicated on the existing archaeological or topographic maps.[13] These included farms of various sizes, frequently containing presses, terraces, field walls, enclosures, wells, and irrigation works. Pottery and coins collected from the majority of sites belonged to the 1st–7th century A.D., indicating that the development of this agricultural landscape coincided with the period of Roman rule in North Africa.

For the purposes of this paper, I wish to focus on the evidence in sector 1 (Fig 4) which proved to be the most complete in terms of the surviving archaeological remains. This sector was also the homeland of the *Musuni Regiani*, an indigenous tribe.[14] The topography of this 20 km^2 study area is

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tion of Rural Settlement in the Cillium-Thelepte region (Kasserine, central Tunisia)', *L'Africa-Romana* 6 (1989) 387–402; R.B. Hitchner et al (1990) 'The Kasserine Archaeological Survey—1987', *Antiquités africaines* 26 (1990) 231–60.

[13] See, in particular, for the region of *Cillium* and *Thelepte* R. Cagnat and A. Merlin, *Atlas archéologique de la Tunisie*, IIe serie (1914–32), feuilles XLVII (Kasserine) and LIII (Feriana).

[14] See H. Dessau, ed. (1896) *Inscriptiones Latinae Selectae* (Berlin: 1896) 9393 (= R. Cagnat, A. Merlin, and L. Chatelain (1923) *Inscriptions Latines d'Afrique* (Paris: 1923) 102) and eadem (1925) *Corpus Inscriptionum Latinarum* (Berlin: 1925) 8.23195 (=*Inscriptions Latines d'Afrique* 103).

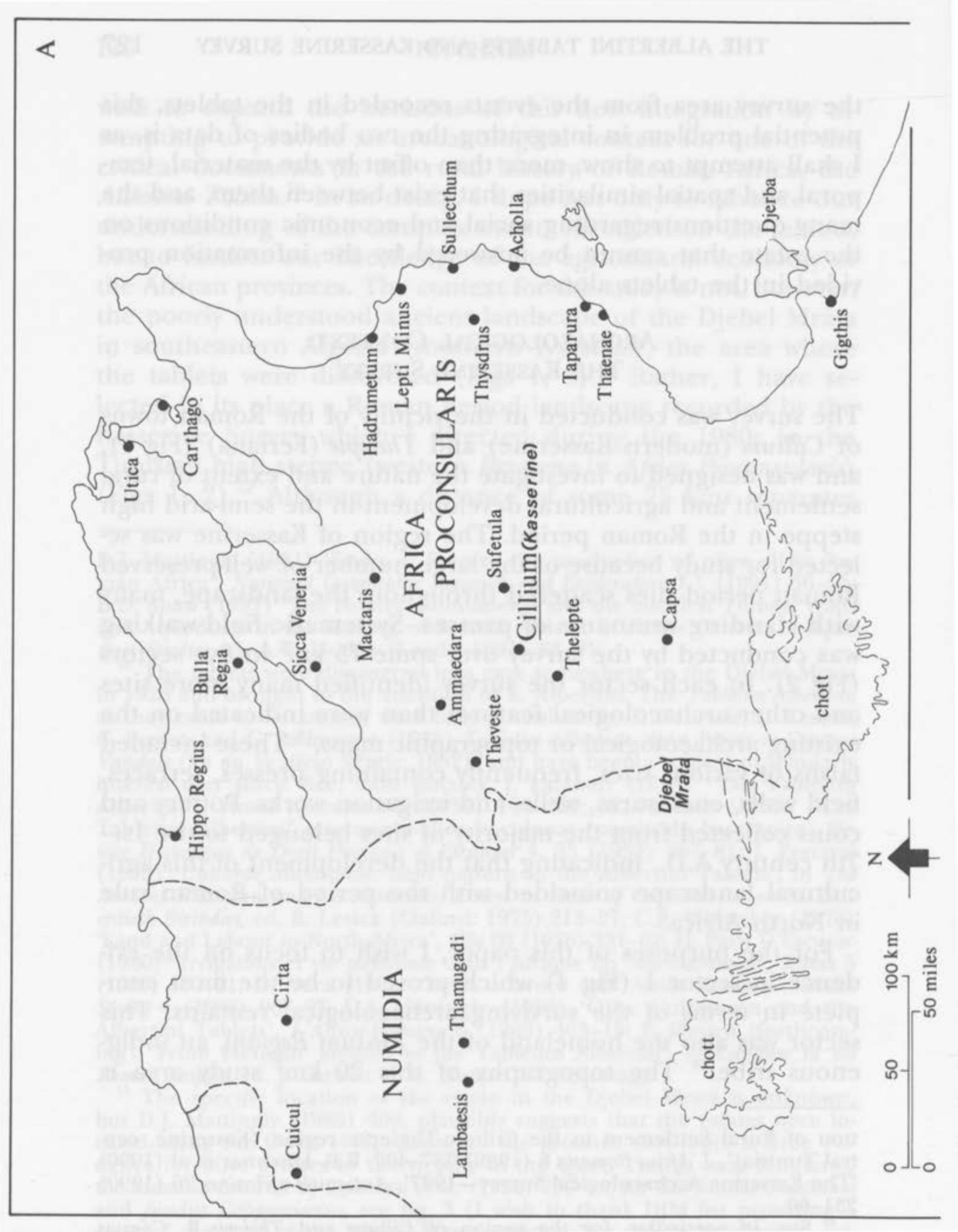

Figure 1. Map of the provinces of Africa Proconsularis and Numidia showing the location of the Kasserine Survey and the Djebel Mrata, the general location of the estate of Fl. Geminius Catullinus mentioned in the *Tablettes Albertini*.

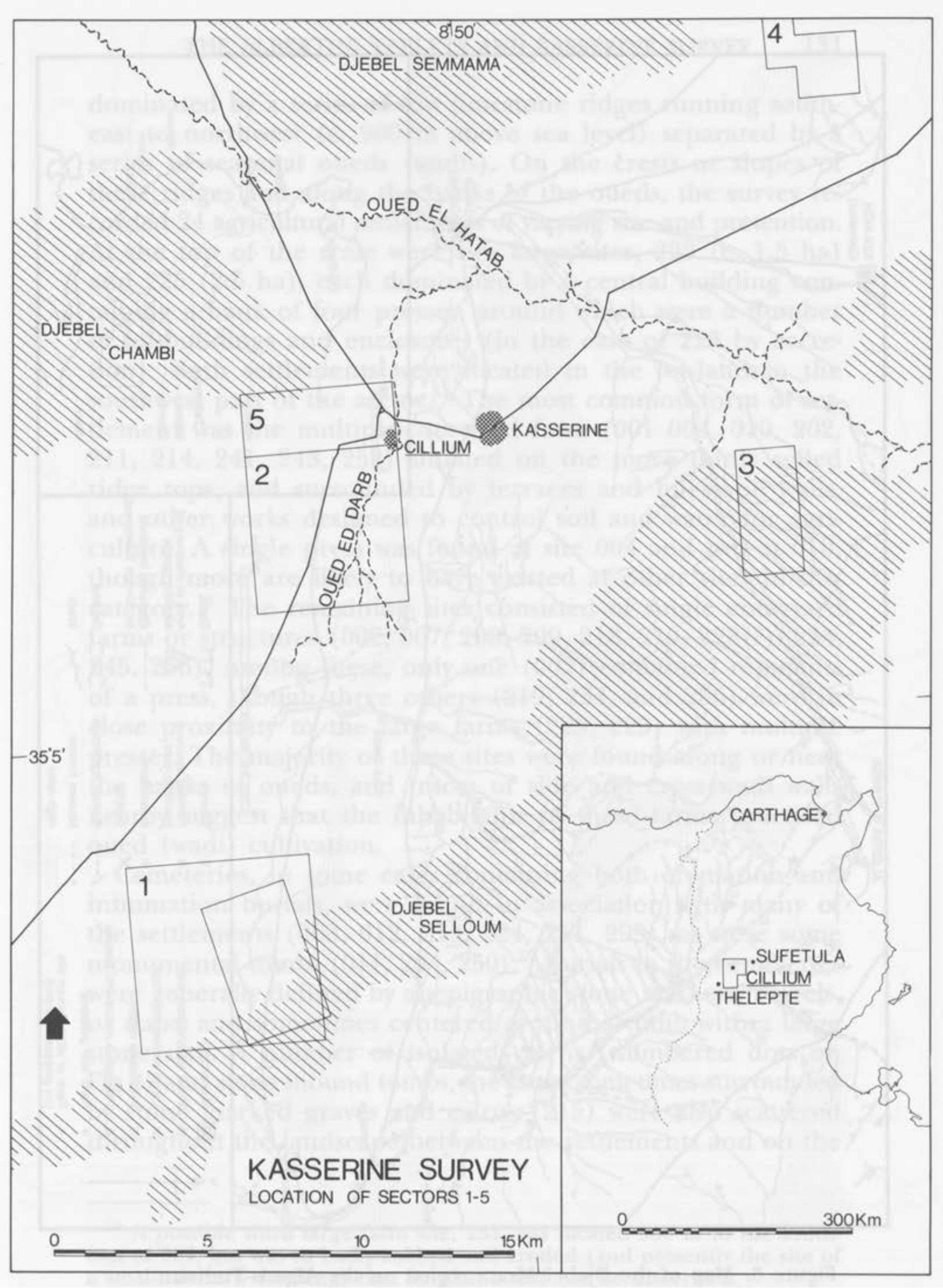

Figure 2. Map of the Kasserine region showing the location of the five survey sectors. Sector 1 is at the bottom of the map.

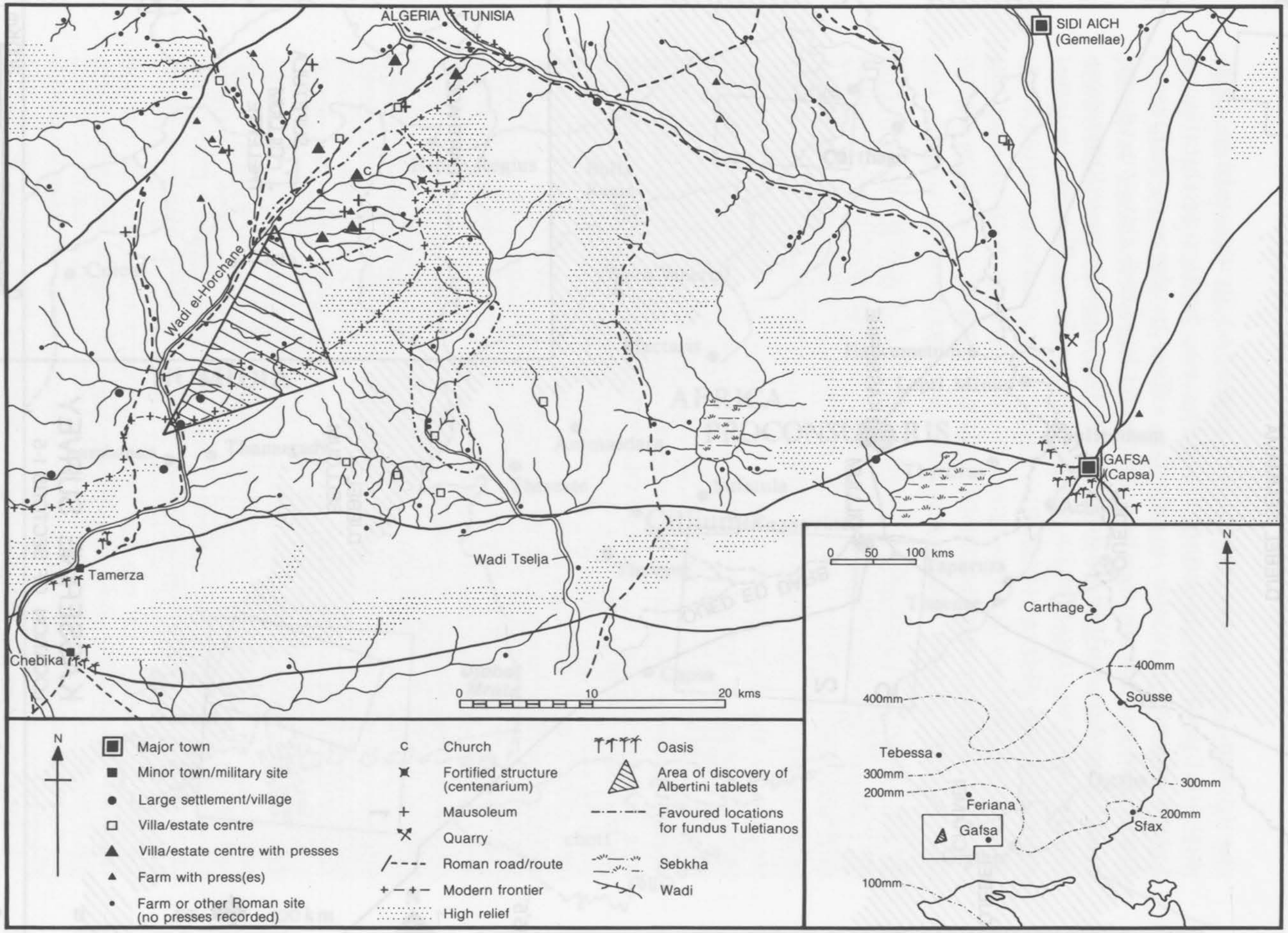

Figure 3. Map of the Djebel Mrata region on the Algero-Tunisian border. Note the area of discovery of the *Albertini Tablets* and Mattingly's suggested locations for the *fundus Tuletianos*. (from Mattingly 1989).

dominated by a series of low limestone ridges running southeast to northwest (c. 900 m above sea level) separated by a series of seasonal oueds (wadis). On the crests or slopes of these ridges and along the banks of the oueds, the survey recorded 24 agricultural settlements of varying size and pretention. At the top of the scale were two large sites, 223 (c. 1.5 ha) and 225 (2.5 ha), each dominated by a central building containing a bank of four presses, around which were a number of outbuildings and enclosures (in the case of 223 by accretion). Both settlements were located in the lowland in the southwest part of the sector.[15] The most common form of settlement was the multiple-courtyard farm (001 004, 010, 202, 211, 214, 241, 243, 252) situated on the more thinly soiled ridge tops, and surrounded by terraces and hill-slope walls, and other works designed to control soil and water for agriculture. A single press was found at site 004 and two at 010, though more are likely to have existed at other sites in this category.[16] The remaining sites consisted of single courtyard farms or structures (002, 007, 208, 209, 218, 219, 228(?), 234, 245, 266). Among these, only one (002) contained remnants of a press, though three others (219, 234, and 266) were in close proximity to the large farms (223, 225) with multiple presses. The majority of these sites were found along or near the banks of oueds, and traces of side and cross wadi walls nearby suggest that the inhabitants of these farms practiced oued (wadi) cultivation.

Cemeteries, in some cases containing both cremation and inhumation burials, were found in association with many of the settlements (008, 012, 213, 224, 251, 299), as were some monumental tombs (011, 204, 250).[17] Burials in the cemeteries were generally defined by anepigraphic stone markers, barrels, or slabs, and sometimes centered around a tomb with a large stone slab. A number of isolated cairns (numbered dots on Fig 4) and stone mound tombs, the latter sometimes surrounded by stone marked graves and cairns (215) were also scattered throughout the landscape between the settlements and on the

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[15] A possible third large farm site, 233, was located 500 m to the southeast of 223, but was so badly robbed and eroded (and presently the site of a small modern dwelling) that its plan could not be discerned.

[16] Hitchner and Mattingly (1991).

[17] The cemeteries and mausoleums were situated on what appear to be the physical boundaries of the various properties.

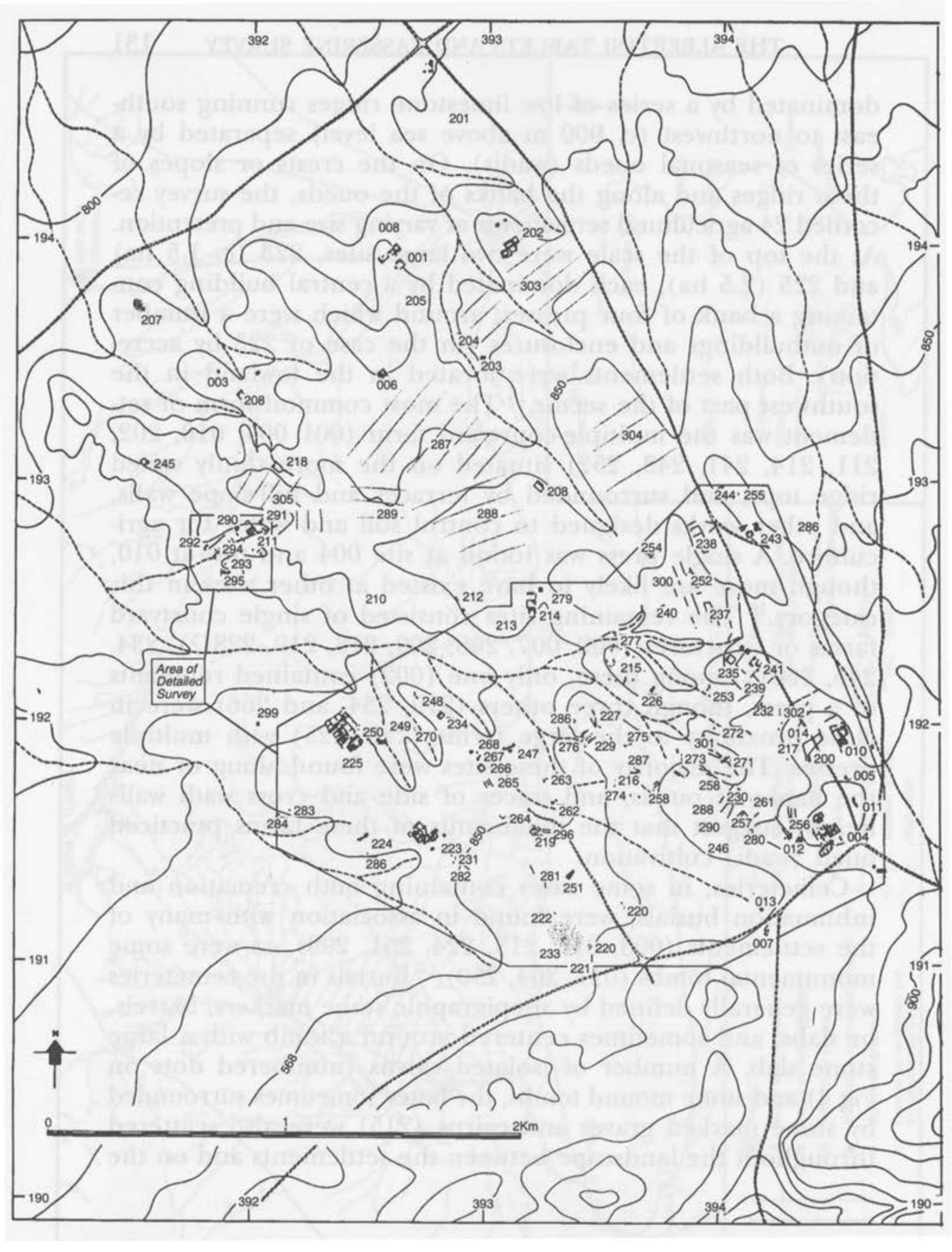

Figure 4. The Roman period landscape in Sector 1 as recorded by the Kasserine Survey. The terrain to the northwest of the Area of Detailed Survey was not shown in previous publications of the project.

ridges. These undoubtedly belong to an indigenous burial tradition anterior to the Roman period cemeteries noted above.[18]

The terraces recorded by the survey were visible mainly as a series of stone wall footings cut into the slopes of the ridges and the gently sloping ground at the northern end of the sector. Traces of cross and side oued walls were also observed along the edges of some of the oueds, though most evidence of oued farming is now completely lost due to erosion and alluvial redeposition. Run-off cisterns, feeder walls, sluices and other evidence of run-off irrigation have also been identified in this remarkably fossilized agricultural landscape. As I have already indicated, physical connections between certain field systems and settlements are apparent.[19] This suggests that we are dealing with carefully-defined property units, though it is clear from the close proximity of one field system to another throughout the sector, and the heavy emphasis on runoff cultivation, that productive alliances and obligations must have existed between the inhabitants of the various settlements.[20]

The eleven presses known from sector 1 and the extensive evidence of terraces and other forms of irrigation indicate that olive production for the purpose of producing oil on a very substantial scale formed a core element of local agricultural production.[21] Olive oil production on the two large farm sites, 223 and 225, is estimated to have been as high as 40,000 – 80,000 liters in peak years requiring potentially as many as

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[18] R.B. Hitchner et al. (1990) 'The Kasserine Archaeological Survey—1987', *Antiquités africaines* 26 (1990) 240–42.

[19] Some examples: settlement 241 and field systems 235–36; 252 and terrace system 237; settlement 004 and the network of terraces and irrigation works along the ridge to the northwest of the site; 256–59, 273, 275, 276, 286, 216, 230, etc.

[20] Stone steles, possibly serving as property boundaries, were found along the oued edge opposite (north of) site 219, (263), and at the western limit of the field and oued irrigation system connected to site 211 (292).

[21] Three rotary querns and a large mill, indicative of local cereal production, were recovered by the survey in the sector. A small number of carbonized grape pits and almonds has also been collected from on top of one of the presses at site 225, but the small size of the sample and uncertainty over the date of its deposition renders risky any conclusions about the extent of vine and almond production in the sector during the Roman period. Stock raising, on the other hand, may have competed with olive production, to judge from the large number of enclosures found among the settlements, and literary evidence, indicating such pastoral activity in the region, see R.B. Hitchner (forthcoming-b) 'Image and Reality: The Changing Face of Pastoralism in the Tunisian High Steppe in the Roman and Late Antique Periods', in *Landuse in the Roman Empire*, eds. J. Carlsen et al. (Rome: forthcoming).

10–20,000 mature trees spread out over 100–400 ha.[22] On the basis of these estimates, I have suggested[23] that these two sites were production centers for an estate that included sector 1.[24]

The survey evidence from sector 1 is perhaps the most detailed archaeological record presently available of a Roman period landscape in the Maghreb, and thus stands as a new and unique source of comparative data with which to reassess the documentary record on the agricultural economy of the North African provinces.

HISTORICAL TEXT: THE *ALBERTINI TABLETS*

The *Albertini Tablets* is the name given to a set of archival documents inscribed on wooden tablets dating to the late 5th century A.D. All but four of the 33 surviving documents concern the sale of title to agricultural parcels (*particellae agrorum*) located mainly on the *fundus Tuletianos*, one of four such *fundi* mentioned in the acts, under the *dominium* of a certain *Flavius Geminius Catullinus*, probably an absentee landowner.[25] The parcels are identified specifically as 'Mancian cultivations' (*culturae mancianae*), indicating that they were lease-holds regulated by the terms of the *lex Manciana*, a late 1st century A.D. law which had as its general purpose the development of previously uncultivated lands on estates in North Africa.[26] Of the

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[22] Mattingly in Hitchner et al. (1990) 255.

[23] Hitchner (1989); Hitchner et al. (1990).

[24] Site 223 with its projecting corner towers and inset entrance along its northeast side was perhaps the residence of the *conductor* of the estate. While we cannot be certain that the settlements in Kasserine sector 1 constitute a *fundus* or even part of one, their proximity and suggested productive links to the large press sites 223 and 225 are consistent with a pattern of smaller farms orbiting larger centralized farms often with multiple presses found throughout the high steppe (Mattingly (1988) Hitchner (1989)). Among the settlement groups of this type recorded or observed by the Kasserine survey, moreover, there is virtually no deviation from the pattern of land organization employed in sector 1.

[25] D.J. Mattingly (1989) 'Olive Cultivation and the Albertini Tablets', *L'Africa-Romana* 6 (1989) 404. The other documents concern a dowry (I), the sale of a slave (II), the sale of a press (XXXI), and a tablet of accounts (XXXIII). The other three estates are identified as the fundus *Magula* (IV–31), the *fundus Gemiones* (XXIX, 7), and *fundus Capprarianus* (II 3–4).

[26] The literature on the this enigmatic law is considerable; but see in particular D.P. Kehoe (1984a) 'Private and Imperial Management of Estates in North Africa', *Law and History Review* 5 (1984) 241–63; D.P. Kehoe (1984b) 'Lease Regulations for Roman Imperial Estates in North Africa', *Zeitschrift fur Papyrologie und Epigraphik* 56 (1984) 193–219; D.P. Kehoe (1985) 'Lease Regulations for Roman Imperial Estates in North Africa', *Zeitschrift*

26 acts where the names of the purchasers survive, *Geminius Felix* is cited as the new titleholder twenty times, *Geminius Cresconius* and *Cresconia* four times, and *Julius Victorinus* and *Donata* twice. It is generally agreed that the *Geminii* were either kinsmen or freedmen of *Flavius Geminius Catullinus* acting as *conductores* (agents) for his estates.[27]

The parcels for sale were located in areas within the *fundi* known as *loci* or *agri* which, to judge from the topographic names applied to them and their location in the landscape, were areas of land along upland oueds.[28] The terminology used for the parcels indicates that they were small (*particella agri*; *firustellum*: XI, XIV, XXIII; *massa*: V, XXIV; *aumae*: IV, XIX; *cuneus*: XXI). Some were walled or terraced (*gemiones*: V, VII, XIII, XV *et passim*),[29] others were flooded (*aquarium*: IV, XV, XIX) or provided with what were evidently permanent waterworks (*cum lateretis aquariis vergentisque*: V, VII; *cum lateretis et aquariis*: III; *cum aquariis et vergentibus*: XI).

The majority of the plots were cultivated with olives, though figs, almonds, pistachios, and vines are also mentioned.[30] Groups of parcels were frequently worked by members of a single family;

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fur Papyrologie und Epigraphik 59 (1985) 151–72; D.P. Kehoe (1988) *The Economics of Agriculture on Roman Imperial Estates in North Africa* (Gottingen: 1988).

[27] Mattingly (1989) 404–5; P. Ørsted (forthcoming); The domain of *Catullinus* may have been one of the so-called emphyteutic estates cited in the Theodosian Code, see P. Ørsted (forthcoming); H. Pavis D'Escurac (1980) 186–88.

[28] J. Lambert (1953) 'Les Tablettes Albertini:, *Revue Africaine* 97 (1953) 214–16; Mattingly (1989) 408–11. The *loci* or *agri* mentioned in the tablets all belonged to the *fundus Tuletianos.* They are identified as: *in aggarione, in aumas, in buresa, in buinac, post centenarium, domi veteres, in pullatis, sela, in sicillione, pars de susa, tres arbores, in valles,* see Courtois et al. (1952) 199–201. More than two-thirds of the parcels were in five *loci* and three *loci* incorporated half of the parcels cited in the documents (*buinac,* 9 plots; *sicilliones,* 7 plots; *pullatis,* 5 plots; *aggarione,* 4 plots; *domos veteres, sela, aumas,* 2 each; the rest, 1 each). The term *locus* was also used in a more restricted sense to designate a piece of land; i.e., *locus agri* (Acts IIII, XVI, VII, XXIV). Percival (1975) 216–21, interpreted the *loci* and *agri* as referring to open-field systems similar to those found in the early medieval landscape of northern Europe, but the radically different geographic, climatic, and socio-economic conditions in the Djebel Mrata make this comparison unlikely.

[29] These parcels were sometimes located in oueds, see Acts V, *gemiones in siciliones in gunfliones*; XV, *gemio de riv.*

[30] C. Courtois et al. (1952) 201–2; Mattingly (1989) 410–12. The small size of the plots is also revealed by the fact that most of the sales involved parcels with fewer than 10 trees, but in any case no more than 37 (Act VI). See Mattingly (1989) 411.

some individuals and families also owned parcels in different *loci* and even in different *fundi*.[31]

The causes for the sales of the parcels have elicited much discussion, but three pieces of evidence in the acts suggest that the transactions resulted from the failure of tenants to cultivate or maintain in cultivation their plots as a result of changes in their personal life and/or the conditions of the plots themselves.[32] First, although the majority of plots for sale were cultivated, some were not, indicating that while the fruits of cultivation were perceived to provide added value, it was ultimately the right to work the land, whether cultivated or not, that was at issue.[33] Second, three of the acts involved the sale of plots by widows (XV, XXI) and possibly an elderly male and his daughter-in-law (XXIII: in the latter case the sale was of uncultivated plots).[34] Third, the quoted sale prices for all the parcels are low. Although reservations have been expressed about the validity of the sale figures,[35] it should be remembered that late Roman *annona* assessments accorded low value to land and olive groves in hilly or mountainous areas.[36] Other factors such as the marginal arable potential of the terrain on which the parcels were situated (and indeed the entire estate),[37]

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[31] See Acts III, VI for families owning properties near one another and in different parcels. The *Geminii* also own parcels in different *loci*.

[32] Courtois et al. (1952) 210–11 and Mattingly (1989) 414 discuss the personal nature of the problems facing some of the sellers.

[33] See Acts V, XV, XXI, XXIII for reference to parcels for sale that evidently have neither crops nor irrigation technology.

[34] Widows mentioned as sellers in the documents include *Siddina*: XIII, XV, XVII, XXXI, XXXIII; *Adeudata*: XI, XVIII, XXXIII; *Fotta*: XXIV, and *Preiecta*: XXII, XXVII, XXXIII. There are numerous references to *heredes*.

[35] P. Ørsted (forthcoming); Mattingly (pers comm.) The price of an olive tree was 15/16 *folles*, a fig three 50 *folles*, see Acts V, VI, IX, XIV, XVIII and P. Grierson (1959) 'The *Tablettes Albertini* and the Vale of the *Solidus* in the Fifth and Sixth Centuries A.D.', *Journal of Roman Studies* 49 (1959) 74. Courtois et al. (1952) 204–05, employed the dubious method of comparing the prices in the documents with those in North Africa in the modern period. Ørsted (forthcoming) advances the view that the Acts are not actually sales, but prearranged prices for the sale of two-thirds of the production of the tenants on the *culturae mancianae* to the *conductor* or his *vilici*. But this interpretation is difficult to sustain given the form of the contract attested in the tablets.

[36] See the *Syro-Roman Lawbook* in *Leges saeculares* 121 (FIRA2 II, 795–6).

[37] This is vividly illustrated in one case in the vain attempt to cultivate almond trees in a flooded parcel indicating that ground water resources were wholly inadequate for this crop. While some tenants were producing enough olives (one family owned as many as 70 olive trees (Acts VII, X, XII, XIV, XX, XXII)) to own a press implied that olive oil yields exceeded, at least occasionally, any rent and subsistence requirements (Hitchner forth-

and the remoteness of the Djebel Mrata from the price-setting market towns along the coast must also be taken into account.[38]

To sum up, the land sales in the *Tablettes Albertini* involved the buy-back of the rights to work small plots of leased land in specific parts of estates in the Djebel Mrata by agents of the estate owner. The plots were sold because they were not being cultivated and/or could not be cultivated, or if cultivated, could not be maintained by the tenant-sellers for personal or agricultural reasons.[39] While it is true that the terms of the *lex Manciana* called for forfeiture of all rights to cultivation of a leased parcel after two years if the latter was not maintained,[40] the rigor of this stipulation dating from the 2nd century must be set against the reality of conditions on the Djebel Mrata estate in the late 5th century in which a group of probably bound tenants (*coloni*) belonging to a clan or some equivalent social group (the *Iulii*) are attempting to carve out an existence on land at the southern limits of the arable zone. From this perspective the transfer of title between individual parties belonging to this clan or group and a member of the *familia* of *Fl. Geminius Catullinus* (mainly *Geminius Felix*) would seem best explained as an accepted legal recourse applicable under certain circumstances (death, infirmity, indebtedness, soil infertility/exhaustion, drought). This remedy, I suggest, lifted tenants from their responsibility to cultivate specific plots on the estates, which they or their ancestors contracted to work

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coming-a). The marginal nature of the land—a condition reflected in both the emphasis on irrigated parcels and preference for the olive and fig, the only cultivars that could produce under the harsh, arid environment of southern *Numidia*—militates against the assumption that such surpluses were achievable indefinitely. Indeed, if the parcels under olive cultivation mostly contained mature trees, yields, though possibly spectacular early on, are likely to have declined from rapid soil exhaustion on such marginal lands (on the question of varying yields through time see D.J. Mattingly (forthcoming) 'Regional Variation in Roman Oleoculture: Some Problems on Comparability', in *Landuse in the Roman Empire*, eds. J. Carlsen et al. (Rome: forthcoming)). Such declines in yield would certainly have contributed to the decision by the tenant/sellers to divest themselves of the rights to the parcels and their produce. For a comparable situation see Pliny, *Epistles*. 3.19.

[38] Grierson (1959) 76. Note also that the tax figures for *Numidia* were reduced by Valentinian III to one eighth of the previous total in 445, see *Val. III, Nov. XIII*, p. 445; A.H.M. Jones (1964) *The Later Roman Empire, 284–602* (Norman OK: 1964) 462–64.

[39] See below note 27 for discussion of potential problems in olive cultivation on the estate.

[40] See *Corpus Inscriptionum Latinarum* 8. 25902; Mattingly (1989) 414.

at some point in the past, but did not result in the displacement of the sellers (tenants) from the estates.

There remain, however, many unanswered questions regarding agricultural organization and property relations on the estate of *Catullinus*. Were the *loci* and *agri* major subdivisions of the *fundi* or merely units of land within some larger configuration on the estate? How much land within the *loci* and *agri* was taken up by Mancian cultivations? What did the latter look like on the ground and how were they organized in the landscape? Where did the tenant/sellers reside on the estate? And were there other forms of land tenure on the estate? To answer these questions, I would like to return to the evidence from sector 1 of the Kasserine survey.

INTEGRATION OF THE DATA FROM KASSERINE SECTOR 1 AND THE *ALBERTINI TABLETS*

There are, as should be evident by now, broad parallels between the archaeologically recorded community in sector 1 and the Djebel Mrata *fundi*. Both were long-term products of the Roman period expansion of an essentially indigenous irrigated tree-based agrarian economy into the semi-arid steppes and uplands of southern *Africa Proconsularis* and *Numidia*.[41] Both, moreover, were built on the labor of a cohesive, native community organized for productive purposes into a formal network (the *fundus* in the case of the Djebel Mrata) of small agricultural units. Most importantly, there appears to be a close correspondence between the landscape organization of the *fundi* and specific agrarian features in sector 1. This is most evident in the terraces and field systems which seem to closely approximate the description of the various types of *particella agri* in the tablets. The terraces associated with sites 010 (014), 241 (235, 236), 252 (237, 238), for instance, look to be the equivalent of *gemiones*. Likewise, the complex system of crossoued walls, wells, and sluice works at site 211 (291, 293, 294, 295) and the hydraulic works along the ridge top to the northwest of 004 (216) are almost certainly examples of *aquaria*, *aqua putei* and *cum lateretis aquariis vergentisque suis*. Instances of *massae* and *firustella* are more difficult to discern, but the small plots on the west side of the main building at site 004

[41] Mattingly (1989) 414–15; B.D. Shaw (1991) 86.

may fall into this category.[42] There is likewise a remarkable congruence between the description of the *loci* and *agri* in the tablets and the clusters of terraces within specific sections of the sector 1 landscape generally associated with individual settlements. The latter pattern is especially interesting for it suggests that the *loci* and *agri* on the Djebel Mrata *fundi* were not mere topographic subdivisions of the estate, but agricultural units of varying size comprised of a central farm or settlement and a series of fields, parcels of which were worked as Mancian cultivations by tenants residing on the units. This hypothesis is strengthened by references in two of the acts (VIII, IX) to *loci* that have buildings on or near them, specifically, an old house, (*domus veteres*) and *centenarium*[43] or purpose-built structure (such as the building at Kasserine site 225), and the map of the Djebel Mrata prepared by Mattingly showing a Roman period settlement density virtually identical to that found in sector 1 (Fig 3).[44] At any rate, the subdivision of the Djebel Mrata *fundi* into agricultural units would help to explain the concentration of family parcel holdings within specific *loci.*[45] It may also be inferred, on the basis of the proximity of the settlements to one another in sector 1, that the various *loci* and *agri* on the estates owned by *Catullinus* tended to be adjacent to one another, an arrangement that would accord well with the evidence that some tenants owned parcels in more than one *locus* or *ager.*

It remains to address the question of how much of the cultivated land on the estate of *Catullinus* consisted of *culturae*

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[42] The two oueds running southeast to northwest through sector 1 would fit the description of a *rivus*; their junction to the southwest of site 214 is perhaps a *gunfliones*. Erosion in these oueds has destroyed most of the parcels which once undoubtedly existed within and along their banks.

[43] This was probably a fortified farm, see Mattingly (1989) 409 n. 30.

[44] For references to these settlements see above note 3. The absence of notice in the act to buildings on the majority of the *loci* or *agri* mentioned in the tablets need not imply that they lacked settlements as farms are often as not identified by the quality or nature of the terrain they occupy as by the buildings themselves.

[45] A parallel to the system of land organization suggested here is the henchir in 17th and 18th century Tunisia which was a land unit comprised of a farm, fields, plots, and other land subdivisions. Interestingly, the plowed surface in a particular field within a henchir was designated by the term *mashia*. When a farmer leased a piece of land or property, he did not plow all of it, but only a number of *mashia*. It is also worth noting that a Tunisian farm would never define a piece of land as a *mashia*, only by its topographic or toponymic name. See L. Valensi (1985) *Tunisian Peasants in the Eighteenth and Nineteenth Centuries* (Cambridge: 1985) 128–32.

mancianae. While the evidence from sector 1 can offer little concrete evidence on this question, it is worth noting that there seems to be no distinguishable difference in the types of parcels recorded throughout the sector suggesting that if different forms of tenure existed on the probable estate, they were not based on principles different from those used to define Mancian plots. The situation seems to be much the same in the Djebel Mrata *fundi.* There were, to be sure, some significant advantages to cultivators working under terms of the *lex Manciana.* Specifically, the law offered attractive terms for tenants to develop marginal arable land.[46] It also, as the sales of the parcels reveal, provided an escape clause for tenants who were no longer able to maintain the leased parcels.[47] It would thus seem reasonable to conclude that the majority if not all of the parcels on the *fundi* owned by the *Geminii,* as well as those in sector 1, were *culturae mancianae.*[48] At the very least, the evidence shows that the Mancian cultivations, at least in the steppe and uplands of the south, were little more than small arable tracts carved indiscriminately out of fields or terrain associated with farms or other agricultural property units belonging to an estate.

Larger Implications of the Study

The similarities in the agricultural regimes practiced in the Kasserine region and the Djebel Mrata in the Roman period offer not only a fuller contextual framework for resolving problems in the understanding of events on the estate of *Geminius Catullinus,* but contribute to a growing body of evidence for different agricultural patterns from north to south in the African provinces broadly comparable to those practiced in the region in the modern period.[49] It would now seem likely, as I have suggested above, that the semi-arid, southern half of the provinces of *Numidia, Africa Proconsularis* and the whole of *Tripolitania* was chiefly devoted to arboriculture (especially olives)

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[46] Mattingly (1989) 413.
[47] This clause will have been especially favorable to tenants if it did not result in their departure from the estate, Indeed, by the Late Antique period, this could well have been viewed by estate owners as an effective non-coercive mechanism for keeping tenants on estates.
[48] Mattingly (1989) 413.
[49] Valensi (1985) 61–182.

and livestock production.[50] This agropastoral regime, unlike the predominantly cereal-based agrarian economy of the river valleys, Tell, and plains in the north,[51] was probably not organized on the Italian model of centuriated estates, but on an essentially indigenous form of irrigation agriculture.[52] A number of factors contributed to this development, chief among them the growing demand at Rome and elsewhere for African olive oil in addition to cereals, the availability of indigenous labor in the form of largely rural tribal communities (*civitates*), and massive capital investment by wealthy landowners including the emperor.[53] While it is not yet clear how extensively the *lex Manciana* was deployed as a general incentive to the development of the southern agricultural economy in contrast to its more limited role in the north as a spur to the cultivation of *subseciva,* its survival into the late 5th century as perhaps the primary or sole governing mechanism for the tree-based cultivation of land on an estate at the southern extreme of the irrigation agriculture zone stands as either a remarkable legal curiosity or a gauge of the universal appropriateness of this law to the development of agriculture in the challenging environmental, economic, and cultural circumstances of southern *Africa Proconsularis* and *Numidia.*

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[50] P. Leveau (1992) 'L'occupation du sol dans les montagnes Mediterranéennes pendant l'Antiquité: apport de l'archéologies des paysages à la connaissance historique', in *La montagne dans l'antiquité. Actes du colloque de la Sophau, Pau Mai 1990* (Pau: 1992); Shaw (1991) Mattingly (1988), (1989), R.B. Hitchner (forthcoming-b) 'Image and Reality: The Changing Face of Pastoralism in the Tunisian High Steppe in the Roman and Late Antique Periods', in *Landuse in the Roman Empire,* eds. J. Carlsen et. (Rome: forthcoming).

[51] See J. Peyras (1991) *Le Tell Nord-Est Tunisien dans l'Antiquité. Essai de monographie régionale* (Paris: 1991).

[52] Hitchner (1989), (1990); Mattingly (1989). On the arguably fundamental African context of irrigation agriculture in the interior of *Africa Proconsularis* and *Numidia* see also Leveau (1992); Mattingly (1989), and B.D. Shaw (1982) 'Lamasba: An Ancient Irrigation Community', *Antiquités africaines* 18 (1982) 61–103, B.D. Shaw (1984) 'Water and Society in the Ancient Maghrib: Technology, Property and Development', *Antiquités africaines* 20 (1984) 121–73, Shaw (1991). This view was first advanced and developed by J. Despois (1935) *Le Djebel Nefousa (Tripolitaine): étude géographique* (Paris: (1935), J. Depois (1953) 'Géographie et histoire en Afrique du Nord', in *Hommage à L. Febvre I* (Paris: 1953), J. Depois (1955) *La Tunisie Orientale. Sahle et Basse Steppe* (Paris: 1955), J. Depois (1958) *L'Afrique du Nord* (Paris: 1958).

[53] Mattingly (1988), (1989) (forthcoming); Hitchner (forthcoming-a); Hitchner and Mattingly (1991); Shaw (1991).

CONCLUSION

From a methodological perspective, this paper has highlighted the potential of archaeological survey for providing, in contrast to excavation, a wider informational context or 'text' on the social, economic, and technological structures on past societies in particular regions or environments. Even more critical for the purposes of this volume, it has attempted to show how the archaeological 'narrative' of past human interventions in the natural landscape produced by survey data can be effectively integrated with literary or documentary evidence on land tenure, social organization, and agrarian technology. The agrarian history of North Africa is particularly suited—perhaps more than any other region of the Roman Empire—to the blending of survey archaeology and texts (the latter in the form of inscriptions). Indeed, the moment is ripe for a reconsideration of some of the great agrarian inscriptions in the light of the considerable survey work of the 1980s.[54] This paper stands as a modest contribution to that end.[55]

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[54] I refer in particular to the imperial estate inscriptions from the Bagradas Valley, *Corpus Inscriptionum Latinarum* 8.10570, 14428, 14451, 14464, 25902, 26416, 29942, 25943. There is, to be sure, a substantial critical literature on these inscriptions (See Kehoe (1984a); Ørsted (forthcoming)), but virtually all of it overlooks the pertinent archaeological evidence on the estates in feuille XXXIII of the *Atlas archéologique de la Tunisie*.

[55] I wish to thank David Mattingly and Peter Ørsted for many helpful comments via E-Mail and FAX on various aspects of this paper.

MONUMENTS, LAWS, AND ANALYSIS: COMBINING ARCHAEOLOGY AND TEXT IN ANCIENT ATHENS

David B. Small

Introduction

The archaeological record of ancient Athens provides an unparalleled context for scholars interested in the study of society through mortuary analysis. The Kerameikos and other cemeteries have been used by the residents of Athens as burial grounds from the end of the Mycenaean period to the early centuries of the Roman empire. It is no wonder that they had provided an almost irresistable attraction for recent Classical scholars who wish to address questions of religious and social practice, and the larger issues of evolutionary patterns in Athenian social structure through an analysis of Athenian graves. To sweeten the attraction the Athenian textual record also supplies references to sumptuary laws that address funeral behavior. The issuance of two of these laws, dating probably to the early Classical and certainly to the rule of Demetrios of Phaleron in the late fourth century (c. 317 B.C.), actually coincides with documented changes in mortuary elaboration. A better context for analyzing social change could not be found in Greece.[1]

In this chapter I argue however that interpretations of these laws and the larger effort of using textual and archaeological data in social reconstruction are flawed because of methodological shortcomings revealed in the integration of this archaeological and textual material. There are two levels of discussion here. The first is that the cemeteries of Classical Athens

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[1] Some more visible studies are I. Morris (1987) *Burial and Ancient Society: The Rise of the Greek City State* (Cambridge, 1987); I. Morris (1992) *Death-ritual and Social Structure in Classical Antiquity* (Cambridge, 1992); A.J.M. Whitley (1991) *Style and Society in Dark Age Greece* (Cambridge, 1991); R.S.J. Garland (1985) *The Greek Way of Death* (London, 1985); R.S.J. Garland (1989) 'The Well-ordered Corpse: an Investigation into the Motives behind Greek Funerary Legislation', *Bulletin of the Institute of Classical Studies* 36 (1989) 1–15; S.C. Humphreys (1980) 'Family Tombs and Tomb Cult in Ancient Athens: Tradition or Traditionalism?', *Journal of Hellenic Studies* 100 (1980) 96–126. I would like to thank Josh Ober for helpful suggestions in the writing of this essay.

are to be classified as standing monument cemeteries, which have distinct correlations with sumptuary legislation that have been overlooked in current scholarship. The second point stems from observations gained from the first. Scholars of ancient Athens have tended to view its archaeological record as understandable by the application of models of social structure developed from textual evidence. But such privileging of textual material can produce faulty social reconstructions. As I will argue, Athens' mortuary record is not capable of explanation by reference to textual models. It is an independent source of information which challenges textually-constructed models. But, in its challenge, it offers a more sensitive model of Athenian life.

By way of analysis I will first review the context of funeral behavior in ancient Athens, then introduce the mortuary record from Classical Athens and discuss its independent operational features. Then I shall review what we know of sumptuary legislation as it pertains to our material. Following that I turn to current explanations for the appearance of mortuary sumptuary legislation and compare them to explanations that I can offer by correlating sumptuary legislation to observed independent operational features of standing monument cemeteries. In a final analysis I will address the issue of an analytical frame which assumes a dependent relationship between documentary and archaeological data, an assumption that has led to a serious misinterpretation of the relationship between sumptuary legislation and change within Athens' cemeteries.

THE CONTEXT OF THE FUNERAL

Let us first examine the social context for Athens' cemeteries. What follows here is only a brief condensation of various sources. I owe a debt to Garland whose analysis is by far the most exhaustive, and should be consulted by any who desire to study the issue in depth.[2] Death in ancient Greece was a protracted process, whose steps were marked by various observances. The Greek funeral itself had three stages: the *prothesis,* or lying in state; the *ekphora,* or passage to the burial ground; and ceremonies at burial. Upon death, the women of the deceased's family customarily washed the body, which was then laid upon

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[2] Garland (1985)

a bed in the house, dressed in a robe or shroud (soldiers and newly-weds were a possible exception). Scenes of funerals on geometric burial *amphorai*, show that the *prothesis* was also a time for mourning.[3] Many *amphorai* show bereaved children as well as women gathered on each side of the corpse, with the women tearing at their hair or beating their breasts. The main feature of this stage of the funeral was the singing of the funeral dirge. Although we might risk lapsing into a simple positivistic comparison between life and art, it might not be too incautious to argue that the higher percentage of women seen in attendance in these scenes on funeral *amphorai* indicates their dominant role in this process. The 'carrying out' or *ekphora*, is described on funerary ceramics as well.[4] The journey to the grave was accompanied by male and female mourners, who might, at times, have been hired for the occasion. There appear to have been various means by which the transfer to the cemetery was accomplished. What we know from both these ceramic scenes and Homer (*Iliad* 23.131–33), tells us that the body could have been conveyed on a wagon, or carried by pall-bearers. The journey could have been quite noisy, with members of the cortege, accompanied by musicians, wailing and stopping frequently at street corners. Whether the dead was to be cremated or not, the rites and procedures at the grave site appear to have been generally uniform. Those in attendance would have made a small libation for the deceased, sacrificed and burnt birds and other small animals, and set on the grave as offerings, baskets of food, perfume and oil flasks, and pottery.. After the ceremony the women returned to the house of the deceased, while the men probably stayed to complete the erection of the tomb.

Interment did not mark the final act in this death process. Athenians, for example, held that the dead remained in a marginal state, neither in Hades, nor in the world of the living, until the performance of the *triakostia*, or rites performed approximately 30 days after death. This liminal period varied considerably in other communities. It was at least three months long in Gambreion, while the Spartans shortened it to no more than 11 days.

There were several stages in the post burial passage of the

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[3] Good examples can be seen in Bernard Schweitzer (1971) *Greek Geometric Art* (London: 1971) pls. 30, 31, 36, 42.

[4] Schweitzer (1971) pls. 35, 40.

dead to Hades, which were marked by specific ceremonies performed by the living. The first of these was the *perideipnon*, or banquet held at the house of the deceased immediately after burial. There were also subsequent ceremonies performed at the tomb, which consisted of meals, of which the dead were presumably to partake. In Athens some meals were offered on the third and ninth day after internment. There was probably also a final ceremony which marked the end of mourning and the final passage of the dead into Hades, although we do not know what it was called in the Classical period, since our chief sources are Hellenistic.[5]

Although the dead might have finally paid the boatman, this transition to Hades did not mark an end to tomb commemoration. The Athenians held additional commemorative rites for the dead beyond this 30 day period, which were also called *triakostia*, and performed on a monthly basis.[6] They had additional rites known as *eniausia*, a *genesia* or general celebration of the dead, and a less known rite, the *nemeseia*. Commemoration of one's ancestors was further held as an obligation of all citizens of the polis. From at least the Hellenistic period this concern with post burial commemoration appears in the establishment of commemorative foundations that were to ensure that such rites were perpetuated.[7]

THE ARCHAEOLOGICAL RECORD

Having outlined the Athenian funeral context, let me now turn to filling that context with some of its archaeological record. Our knowledge of mortuary material for standing monument cemeteries in Athens comes primarily from two excavations: that of the Kerameikos, and that of Syndagma Square.[8] The

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[5] See numerous references to Hellenistic and Roman period sources in Garland (1985) 38–47.

[6] Garland (1985) 104–20.

[7] Garland (1985) 108–110; see also Humphreys (1980) 122, n. 61; for foundation of Epikouros: Diogenes Laertius 10.16–17; stipulations in a will: *Inscriptiones Graecae* XII 3.330; R. Dareste, B. Haussoullier, and Th. Reinach (1898) *Receuil des Inscriptions Juridiques* (2 vols.) (Paris: 1898) 1.77–78.

[8] For the Kerameikos, see the on-going publications, *Kerameikos: Ergebnisse der Ausgrabungen* (Berlin 1939–90), and the very useful guide, by Ursula Knigge, *Der Kerameikos von Athen: Fuhrung durch Ausgrabungen und Geschichte* (Athens: 1988); For the Syndagma excavations see S.I. Charitonides, 'Anaskaphi Klassikon taphon para tin Plateian Syndagmatos', *Arkaiologiki Ephemeris* (1958) 1–152.

Kerameikos was in use for a long period of time, from at least 1100 to 200 B.C., with later Roman reuse, and the Syndagma Square cemetery is part of a larger cemetery, that of the Diocharian Gate, which was in use from at least 750 to 300 B.C. The graves from the Syndagma excavations range from 425 to 390 B.C. and do not match the full chronological span of the larger cemetery.

Monumental fashion changed over time.[9] In the sixth century tombs were adorned with large *kouroi* or *stelai* with sphinxes or just topped with huge earthen mounds. But there was change around 530 and 500 B.C. As early as 530 B.C. the monuments become less showy, moving from elaborate archaic *stelai* to ones which were shorter and topped with only palmettes (figure 1a). Near 500 B.C. the frequency of production of monuments also drops. With relatively minor exceptions this plain style of tomb decoration remained in effect until c. 425 B.C. when there was a resurgence in elaboration and output.[10] From this period until 317 B.C. *periboloi* signaled a new attention paid to the elaboration of graves. Perhaps fashioned after state funeral monuments, private *periboloi* were plots that were marked by an enclosure wall (figure 2).[11] Individual burials within the *periboloi* were marked by *stelai, naiskoi,* or elaborately carved stone *lekythoi* and *loutrophoroi* (figure 1b). After the sumptuary legislation of 317 B.C. there was an abrupt change in the form of grave marker, with either a *trapeza* or a *kioniskos* (figure 1c + d), both much less elaborate than their earlier counterparts, now marking the grave.

Internal Dynamics

Ancient historians as well as archaeologists who study cemeteries in general, have failed to make an important distinction between cemeteries that mark graves with permanent

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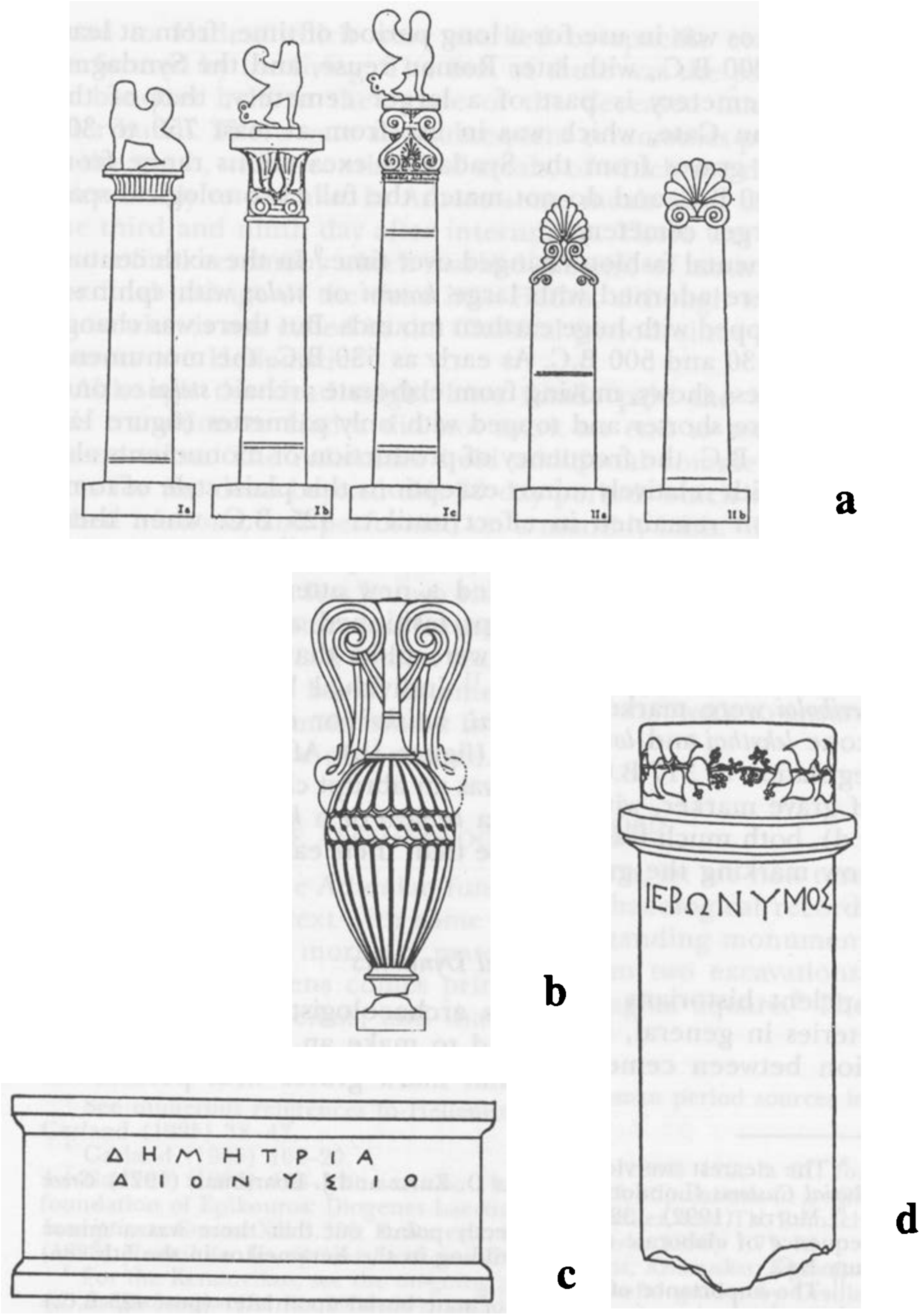
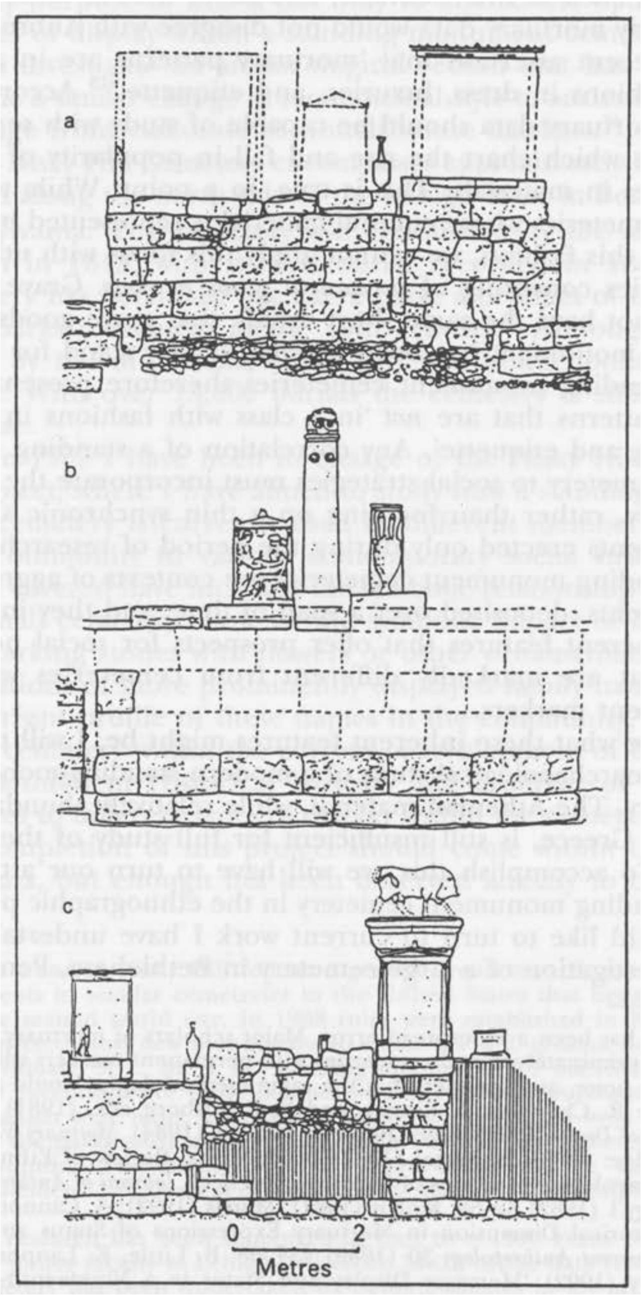
[9] The clearest overview remains D. Kurtz and J. Boardman (1971) *Greek Burial Customs* (London: 1971).

[10] Morris (1992) 132–34, correctly points out that there was a minor sequence of elaborate mound building in the Kerameikos in the 5th century B.C.

[11] The importance of the role of state burial upon later (post 425 B.C.) private tombs is argued by Morris (1992) 128–44; C. Clairmont (1983) *Patrios Nomos, Public Burial at Athens During the Fifth and Fourth Centuries B.C.* (2 vols) (Oxford: 1983); and Nicole Loraux, *The Invention of Athens* (Cambridge MA: 1986).



1. a—Archaic and Classical *stelai* (from Kurtz and Boardman 1971:85); b—lekythos as tomb decoration; c—*trapeza* (from Garland 1982:136); d—*kioniskos* (from Kurtz and Boardman 1971:167).



2. *Perioboloi* from fourth-century Athens: a—tomb of Makareus; b—tomb of Dionysios of Kollytos; c—tomb of Koroibos (from Morris 1992:130).

features, such as the Athenian, and those that do not.[12] Most who study mortuary data would not disagree with Aubrey Cannon's recent assertion that 'mortuary patterns are in a class with fashions in dress, luxuries, and etiquette.'[13] Accordingly then, mortuary data should be capable of study with seriation methods which chart the rise and fall in popularity of different styles in materials. This is true, to a point. While we can study cemeteries whose cultural material is represented by *grave goods* in this fashion, we cannot apply this focus with utility to cemeteries consisting of *permanent grave markers.* Grave markers do not have the same short uselife that grave goods have. Once a monument has been erected, it may stand for centuries. Standing monument cemeteries therefore present mortuary patterns that are *not* 'in a class with fashions in dress, luxuries, and etiquette'. Any correlation of a standing monument cemetery to social strategies must incorporate the whole cemetery, rather than focusing on a thin synchronic slice of monuments erected only during the period of research interest. Standing monument cemeteries are contexts of aggregated monuments, deposited over a span of time, and they incorporate inherent features that offer prospects for social negotiation that are markedly different from cemeteries without permanent markers.

To see what these inherent features might be, I will turn to an ethnoarchaeological study of a modern standing-monument cemetery. The Athenian material, while relatively abundant for ancient Greece, is still insufficient for full study of these features. To accomplish this, we will have to turn our attention to a standing monument cemetery in the ethnographic present.

I would like to turn to current work I have undertaken in the investigation of a large cemetery in Bethlehem, Pennsylva-

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[12] This has been a widespread error. Major scholars of mortuary records have indscriminately mixed cemeteries with permanent markers with those that have none, and assumed that the same methodologies would apply to both. See R. Chapman, I. Kinnes, and K. Randsborg eds. (1981) *The Archaeology of Death* (Cambridge: 1981); J. O'Shea (1984) *Mortuary Variability* (Cambridge: 1984); B. Bartel (1982) 'A Historical Review of Ethnological and Archaeological Analyses of Mortuary Practice', *Journal of Anthropological Archaeology* 1 (1982) 32–58; Morris (1987); Morris (1992); A. Cannon (1989) 'The Historical Dimension in Mortuary Expressions of Status and Sentiment', *Current Anthropology* 30 (1989) 437–58: B. Little, K. Lanphear, and D. Owsley (1992) 'Mortuary Display and Status in a Nineteenth-Century Anglo-American Cemetery in Manassas, Virginia', *American Antiquity* 57 (1992) 397–418.

[13] Cannon (1989).

nia. My purpose in this is not only to discuss the operational features of display within a standing monument cemetery, but also to investigate an archaeological record that has also undergone a similar change in monumental style to ancient Athens: a change from elaborate to nonelaborate markers.[14]

The Nisky Hill Cemetery encompasses approximately 25 acres, located along the north side of the Lehigh River in Bethlehem, Pennsylvania. The cemetery was established by the Moravian church in 1849, with its first burial recorded in 1864. The cemetery has been laid out like a park, a product of the cemetery parks movement of the 19th century.[15] Although established by the Moravians, Nisky Hill is a nondenominational facility. With over 13,000 burials the cemetery is still in current use.

Since 1989 I have been in charge of the Nisky Hill Mortuary Project, where I have aimed to study how a standing monument cemetery is currently used by different members of the local community in various contemporary social strategies.[16] Topics covered have included the symbolic relationship of large and small cemetery markers, the symbolic impact of cleaning and marking stones with flowers or other ornamentation, the correlations of more prominently displayed family names with the current profile of these names in the community, the use of the cemetery within the current redevelopment of the adjacent Bethlehem Historical District, and a survey of visitors' attitudes to different types of display within the cemetery itself. The completion of this project should come within the next few years, but enough has been observed already to be of as-

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[14] The trend in Nisky Hill follows a general trend towards more simple monuments in secular cemeteries in the United States that began shortly after the second world war. In 1968 rules were established in Nisky Hill that restricted most new monuments to either a 1' x 2' ground flush stone for individuals, or c. 30" x 40" standing monument for married couples. The few exceptions are those families who had already purchased monuments, which were more ostentatious before the new rules were enacted.

[15] For the parks movement, see S. French (1975) 'The Cemetery as Cultural Institution: The Establishment of Mount Auburn and the "Rural Cemetery Movement"', in *Death in America* ed. D.E. Stannard (Philadelphia: 1975) 69–91.

[16] My research has been supported in part by a 1990 Franz Fellowship grant to junior faculty at Lehigh University. Much of the data recording in the cemetery has been undertaken by undergraduates in my archaeology classes. I want to thank Dino Ruggiero, Heather Mathews, Ken Tsang, Kara Kudzma, and Dennis Lejeunesse for shouldering much of the daily responsibilities.

sistance in interpreting post-interment use of standing monument cemeteries in general.

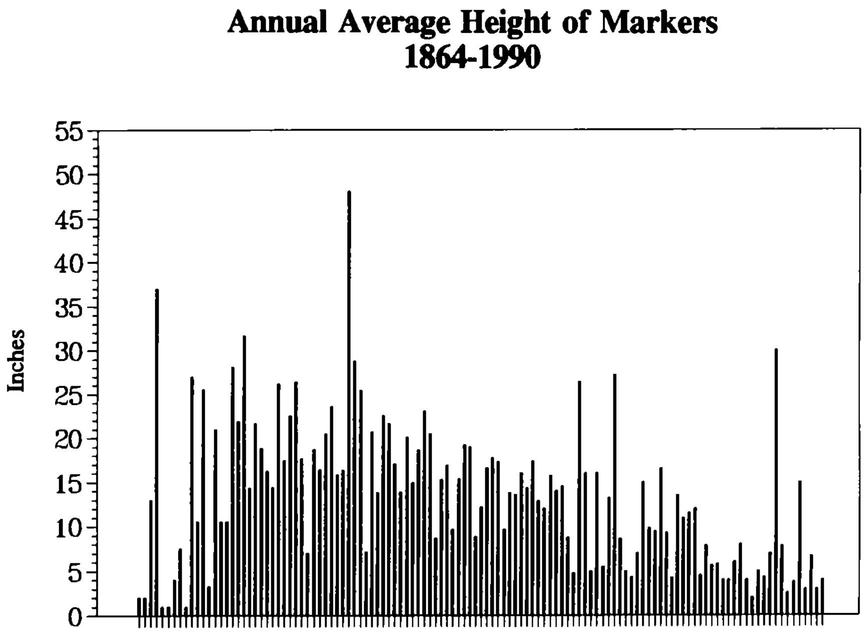
Since the data set is so large, I am going to focus on only one section within the cemetery, section E. This section contains a wide span of standing monuments, from simple ground flush headstones, to large stones marking family plots. It contains 754 burials, with 653 standing monumental stones (100 burials are multiple, represented by a single stone). The earliest burial was in 1871 (six interred died before this date, but their graves appear to have been reburials) and the latest dates from 1990. Although the bulk of present burial activity has moved to the newer eastern sections of the cemetery, section E is not closed, there still being room within plots for future interment. Within the past 30 years the style of tomb decoration has changed from larger stones to smaller, a common dynamic, witnessed in several American secular cemeteries.[17] In 1968 the Board of Trustees of Nisky Hill Cemetery ruled that unless families wanted to duplicate existing headstones, within their own plots, they had to mark new graves with either the 'regulation marker' which was installed 3 inches above ground or a flush marker.[18] Thus, if we were to look at the change in monument elaboration over time, we would see that this section would parallel similar changes in ancient Athens, with smaller and less ostentatious cemetery markers becoming the norm within the last 20–30 years. (figure 3)

Means of Distinction

What we have found in our analysis is that an important inherent characteristic of a standing monument cemetery is that it supplies a dynamic context for status negotiation that begins with the erection of the stones, but continues on in various forms after their placement. In section E we have four principal means by which status negotiation continues after a change to nonelaborate stones: present visual distinction between elaborate family stones and most of the other stones within the

[17] For an analysis of this type of overall change in American cemetery tradition, see R. McGuire (1988) 'Dialogues with the Dead: Ideology and the Cemetery', in *The Recovery of Meaning: Historical Archaeology in the Eastern United States,* ed M. Leone and P. Potter, (Washington and London: 1988) 435–80.

[18] Board of Trustees for Nisky Hill (1968) *Rules and Regulations for Nisky Hill Cemetery* (Bethlehem: 1968).



3. Average change in monument height in section E over time. Years with no monuments erected are not represented.

cemetery; burial association with already-erected elaborate family stones, special options to erect older style stones, rather than the new regulated types; and cleaning.

Current Visual Distinction between Family and Individual Stones

Although we have observed fifteen types of monument in section E, for the following argument we may divide them into two large type families, based upon their most discriminating variable, height. The first group is family stones. A family stone is defined as a stone whose primary purpose is to advertise the family name (figure 4, types 'family' and 'obelisk').[19] It most often bears the name alone and is seldom a grave marker. There are 21 family stones (table 1) which dominate the section in their size relative to the monuments of individuals.

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[19] I am indebted to Lehigh undergraduate, Donna Esposito for permission to publish her typology, which was produced for an archaeology project, and has proven quite amenable to application to the entire research project.

Table 1
FAMILY STONES

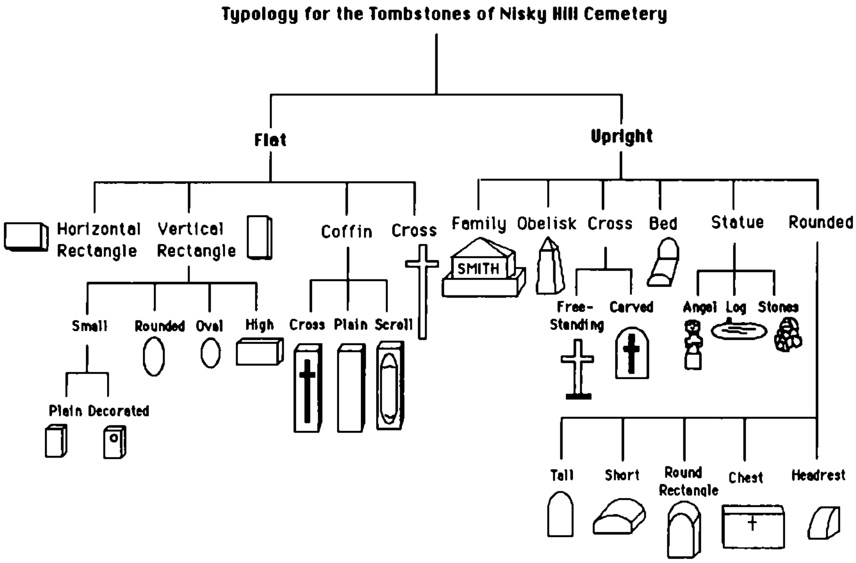
Plot Number	*Family Name*	*Date of Erection*
52	Labergood	1885
34	Weber	1888
42b	Yeager	1897
51	Apgar	1901
47b	Lynn	1905
149	Bartholomew	1907
145	Doutrich	1907
146	Riegel	1909
12+13	Graham	1910
101	Hunsicker	1910
34	Reinheimer	1911
96	Kotz	1913
141	Thatcher	1914
100	Storm	1915
150	Schoenberger	1916
57	Mitman	1918
81	Marstellar	1920
140	Cole	1926
63	Lehr	1931
90	Nostrand	1931
89	Wilhelm	1938

The first was erected in 1888 and the last to be placed here was in 1938.

The other general type is the smaller individual stone. Stones within this category range from 1' × 2' rectangular ground flush stones, with the name of the deceased inscribed on the upper surface, to more vertical monuments with the name of the deceased either on the top of frontal surface (figure 4, all other types except for 'rounded, oval, coffin, cross, bed, statue'). There is a considerable size difference between these two groups. The average of the nonfamily stones is 16 inches and that of the family stones marked at 52 inches.

Even though section E has not seen an interment since 1990, it does not stand mute, but continues to offer a context for status negotiation. Unless one were to walk into section E itself and observe the small stones closeup, which passersby rarely do, section E symbolically advertises these 21 families as more important than the other families or individuals buried within its borders.[20] In their higher position within this section they

[20] A significant part of my current study is a survey of viewers' attitudes to the recognized features within the cemetery. My goal is to develop a



4. Typology of Nisky Hill monuments, devised by Donna Esposito.

identify with additional correlated concepts such as strength and dominance, while the smaller stones take on values of weakness and subordination. Thus, 21 families are fixed within this structure in an advertised hierarchical position. The fact that newer monuments are much less ostentatious than the older stones, dramatically tips the scale of symbolic importance to families who have already erected large family stones. The distinction here is between those who are established already in this context, and new families and individuals, no matter whether they could afford a monumental marker or not, who are not allowed to erect ostentatious monuments.

Distinction by Association with Family Stones

Continued funeral activity can utilize association with these family stones to negotiate an elevated status for those buried after the setting of many of these monuments. If the ceremony

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fine-tuned contextual picture based on an understanding of structural opposition and cultural association. For thoughts on this type of contextual analysis see Ian Hodder (1987) 'The contextual analysis of symbolic meanings', in *The Archaeology of Contextual Meanings*, ed. I. Hodder (Cambridge: 1987) 1–10.

takes place in a plot with an elaborate family stone, those who are conducting the burial are associating themselves with this dominance rather than with the subordinate character of the smaller monuments. This would be the case, even if the buried receives a modern small marker. Since 1968 there have been six burials within family stone plots, five in plot 12+13 (Graham) and one in plot 140 (Cole).

Distinction by Erection of Non-regulation Stones

In the second instance families may choose to duplicate pre-1968 elaborate stones, rather than opting for the new cemetery regulations, if there is an elaborate marker within the family plot that dates from before 1968. Thus post 1968 burials can associate with the older, more elaborate stones rather than with the newer ones. Of the 34 instances where family members have had this choice, i.e. a pre-1968 elaborate stone marker on the plot, in 15 cases they chose to set a more elaborate monument (table 2).

Distinction by Cleaning

The last case of post erection use of the section is somewhat different and is based upon later treatment of the monuments. Because of their location in Bethlehem, a consistent problem with monuments has been discoloration and decay as a result of acid pollution from the Bethlehem Steel Mills. Within a period of 10 years a dark brown patina coats monuments made of hard stone, such as granite, and disintegrates softer stones, such as marble. When stones are cleaned however, they immediately stand out against uncleaned stones, and draw the attention of the visitor. Like height, cleaning highlights selected individuals and families. But it is also distinctly different in that it is an operation which takes place after interment that can be used by relatives to distinguish their monuments from those which have not been so treated.

In section E we have 33 stones that have been cleaned (table 3). Most contracts for cleaning in Nisky Hill are set up so that the stones are cleaned on a scheduled basis, some as often as annually, but never less than five years apart. As far as can be determined, the earliest cleaning contract was initialized in 1931 and the last in 1992. In some cases the monuments were not reused by later family members until, as in the case of the three families in plot 100, 81 years after first erection. Section

Table 2
INDIVIDUAL AND FAMILIES WHO HAD OPTION OF MORE ELABORATE MONUMENT

Plot	*Last Name*	*First Name*	*DoB*	*DoD*	*Regulation?*
4	Ungerer	Jiditha	1896	1971	no
9b	Clemens	Rella	1903	1983	yes
9b	Clemens	Alonzo	1879	1969	yes
10b	Clemens	Catherine	1911	1990	yes
10b	Clemens	Jacob	1908	1972	yes
12+13	Graham	George	1901	1968	no
12+13	Graham	Ethel	1903	1984	no
12+13	Gordon	Ruth	1910	1987	no
12+13	Shea	Marion	1887	1970	no
12+13	Lehnert	Emilia	1885	1970	no
21a	Morris	Frances	1903	1978	no
21b	Liebfried	John	1879	1968	no
21b	Liebfried	Lousie	1882	1974	no
21b	Liebfried	John, Jr.	1920	——	yes
24a	Koch	Mary	1886	1970	yes
24a	Koch	Elmer	1911	1987	yes
22a	Begun	Jeanette	1887	1968	no
22a	Murray	Marion	1888	1968	no
22a	Murray	Annie	1880	1969	no
46b	Boehler	Lewis	1882	1970	yes
49	Daily	Ida	1881	1969	no
68	Hamilton	Arthur	1889	1976	yes
76	Kepfer	Alice	1909	1981	yes
76	Kepfer	Walter	1910	1975	yes
80	Jones	Irella	1878	1969	yes
80	Jones	Maurice	1908	1976	yes
89	Wilhelm	Clemence	1907	1981	no
99	Stout	Robert, Jr.	1895	1974	no
133	Hartley	Orzo	1901	1981	yes
133	Hartley	Helen	1902	1985	yes
138	Whitaker	Helen	1902	1978	yes
140	Cole	Dorothy	1892	1980	no
151	Whetford	Edith	1890	1977	yes
159	Grebs	Cordelia	1924	1969	yes
159	Becker	Hilda	1896	1979	yes

E therefore has remained a context of potential status negotiation for a least three generations.

In short summary, Section E demonstrates the active character of a standing monument cemetery. At least four distinctions are possible within this type of cemetery, which can highlight groups and individuals: distinction established by the presence of large family stones, distinction established by erection and association with large monuments, distinction established by erecting non-regulation markers, and distinction established by cleaning stones.

Table 3
STONES THAT HAVE BEEN CLEANED

Plot	*Name*	*Birth*	*Death*	*Contract*	*Lapse*
12+13	George Graham	1866	1885	1956	71
12+13	James Graham	1830	1909	1956	47
12+13	Eliza Graham	1832	1909	1956	47
12+13	Elizabeth Graham	1863	1945	1956	11
12+13	Mildred Graham	1871	1951	1956	5
12+13	George Graham	1901	1968	1956	0
12+13	Ethel Graham	1903	1984	1956	0
12+13	Emma Graham	1860	1948	1956	8
12+13	James Graham	1858	1918	1956	38
12+13	Ruth Graham	1896	1909	1956	45
12+13	Alexander Graham	1865	1918	1956	38
12+13	Georgianna Graham	1868	1941	1956	15
12+13	Katherine Siebecker	1894	1938	1956	18
12+13	Graham Family		1910*	1956	46
12+13	Ruth Gordon	1910	1987	1987	0
12+13	Marion Shea	1887	1970	1987	17
12+13	Emilia Lehnert	1885	1970	1956	0
12+13	Nellie Towers	1889	1962	1956	0
12+13	Donald Shea	1886	1889	1956	68
89	Mead Beck	1884	1960	1965	5
89	Frederick Wilhelm	1880	1964	1965	1
89	Imogene Wilhelm	1882	1952	1965	13
89	Lydia Beck	1857	1938	1938	0
89	William Beck	1856	1926	1938	14
89	Hazel Beck	1888	1965	1965	0
89	F. Harlan Wilhelm	1906	1959	1960	1
89	Wilhelm Family		1938*	1938	0
90	Nostrand Family		1931*	1931	0
90	Ella Nostrand	1856	1940	1931	0
90	Ray Nostrand	1894	1915	1931	16
90	Benjamin Nostrand	1854	1929	1931	2
96	John Kotz	1829	1013	1946	23
96	Sarah Kotz	1828	1912	1946	24
100	Storm Family		1915*	1992	78
100	Ellen Storm	1856	1923	1992	69
100	Jacob Storm	1853	1915	1992	78
100	Sally Houser	1830	1911	1992	81
100	Alan Chamberlain	1900	1962	1992	30
100	Annie Achenbach	1875	1960	1992	32
100	William Achenbach	1873	1963	1992	29
63	William Lehr	1847	1931	1944	13
63	Sarah Lehr	1853	1933	1944	11
63	Oscar Reese	1894		1944	—
36b	Abigail Seifert	1843	1906	1952	46
36b	Charles Seifert	1839	1916	1952	36
141	Albert Thatcher	1877	1914	1950	36
141	Elsie Thatcher	1882	1946	1946	0
146	Isabella Riegel	1845	1930	1931	1
146	Riegel Family		1909*	1931	22
146	John Riegel	1854	1909	1931	22
150	E. Schoenberger	1857	1916	1942	26
150	H. Schoenberger	1853	1942	1942	0

* date of erection

A Contextual Principle

There is much that could be said about the different ramifications of this type of study, but for this paper I would like to concentrate on a very important contextual principle that has been exposed by the study of these distinctions. *That is, in standing monument cemeteries sumptuary restriction creates a distinction between those with already erected elaborate monuments and those who are now denied that privilege.* This principle is clearly embedded in the first type of distinction noticed at Nisky Hill, visual distinction between elaborate family stones and more modest ones, and the overall decrease in monument height over time. Changes in monument height after 1968 correlate with a separation between new comers and established, rather than a more mixed group of families who, for various reasons, would erect ostentatious or modest stones.[21]

The effect is a crystallization of the already established order, based upon a context which is exclusionary in its opportunity for elaboration. I use the term exclusionary here because the rule was established from the top down, and does not seem to have sprung from any ground swell of sympathy for modest monuments. The fact that nearly half of those who could erect elaborate stones on plots with pre-1968 elaborate monuments did, supports this observation strongly.[22]

Because a structure which distinguishes established and non-established in relation to height or elaboration is now introduced in the context of Nisky Hill by sumptuary regulations, much of the structure of the other noticed distinctions which fall upon these same monuments parallels that of height and elaboration. On-going options for distinction beyond erecting a family stone centered around the chance to erect an elaborate marker, if it duplicates preexisting elaborate monuments on the same plot; the chance to bury within the family stone plots; and cleaning. For cleaning, 8 out of the 9 plots that have cleaning contracts are also family plots. Two of those

[21] Although I use it as a shorthand, the rich versus poor distinction might not be wholly applicable in this case. The poor and marginalized within communities could pool their resources to buy an ostentatious marker. See: M. Parker Pearson (1982) 'Mortuary Practices, Society and Ideology: an·Ethnoarchaeological Case Study', in *Symbolic and Structural Archaeology*, ed. I. Hodder (Cambridge: 1982) 99–113.

[22] This claim is further supported by the trend towards competition in elaboration still witnessed in religious cemeteries within the Bethlehem area.

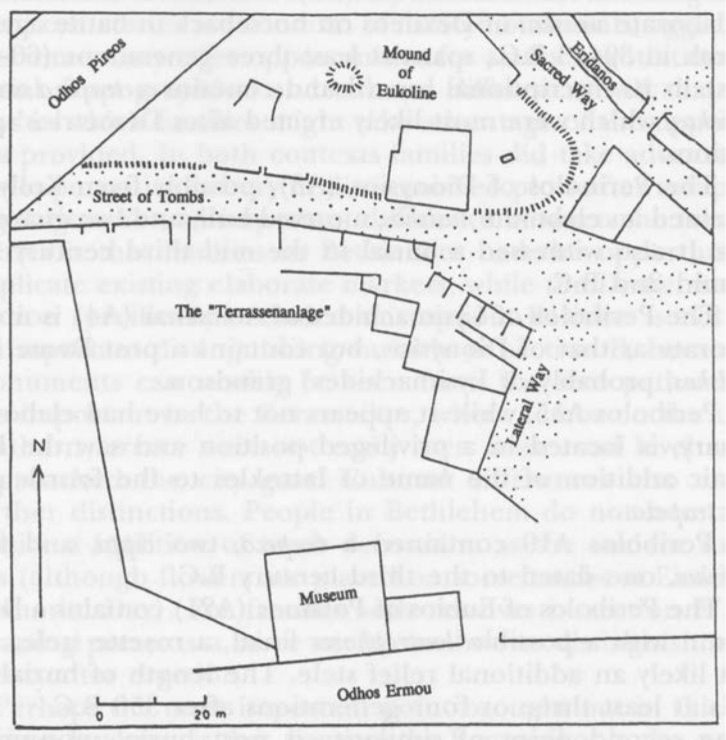
plots, 12+13 and 100, received cleaning contracts after sumptuary regulations went into effect.

The dynamic perspective in the cemetery is important here as well. The families who purchased cleaning contracts did not simply sign them upon the initial purchase of the plot or the erection of the monuments. The move to add an additional symbol to this status game can originate several generations *after* an elaborate monument has been set. The most noticeable case of this dynamic lag comes from plot 100, where a cleaning contract was not signed until the third or fourth generation after the purchase of the plot and erection of the family stone.

THE ATHENIAN COMPARISON

Is it possible to see similar contextual features in ancient Athens? To answer this question, I would now like to turn to an examination of the Kerameikos cemetery (figure 5), when it was probably very similar to Nisky Hill in the pattern of its monument mix, the era of the sumptuary legislation of Demetrios of Phaleron. Like my work in Nisky Hill my focus is on the character of the whole cemetery, rather than just the newer stones, and on the use of the monuments after interment. As I will demonstrate, because the Kerameikos was a standing monument cemetery, it also offered opportunities for continued status negotiation, even after a dynamic shift to smaller monuments.

After 317 B.C. almost all burials in the excavated sections of the Kerameikos were limited to ***kioniskoi*** or *trapezai*, whose visual profile was not as high as the earlier monuments. What then, would the character of the cemetery be after this legislation? In many ways it would have resembled the section from Nisky Hill. First, with new burials limited to less obvious monuments, the older monuments would, in their scale of height and elaboration, have given a dominant position to families who had buried their dead with elaborate markers before 317 B.C. Position of the monument was very important in the Kerameikos. Most of the most elaborate *periboloi* were positioned at the edge of an elevated terrace, close to or at the very lip of the road (Street of the Tombs) that led into the gate of the city. Because that space was essentially occupied by 317 B.C., post 317 B.C. markers were relegated to areas that were not as visible from the entrance road, such as the upper ter-



5. Plan of Kerameikos in late fourth-century (from Garland 1982:127).

race. Again, the cemetery context, like that in Nisky Hill, gives a significant display advantage to families who were able to erect monuments before Demetrios' legislation. The opposition here is between those who have been able to already use this context for status display and newcomers, who were essentially denied access.

Like our study in Nisky Hill, the Kerameikos also demonstrates that people could actively use elaborate monuments for public statements, even after their erection was prohibited. We have two points of evidence. The first is continued burial in elaborate *periboloi* after the legislation of Demetrios was enforced. This operation would tend to reinforce the distinction that has already been marked by size and position. Interest in the use of the Kerameikos after Demetrios has been slight, but even within the area of the Street of the Tombs, six *periboloi*, positioned on the edge of the street, and all, but one, most likely elaborated decorated, were in continued use after 317 B.C.

1. The Peribolos of Dexileos (A1)[23] which was marked with an elaborate *naiskos* of Dexileos on horseback in battle against Corinth in 394/3 B.C., spans at least three generations (60–105 years) in its inscriptional record and contains a *trapeza* and a *kioniskos*, which were most likely erected after Demetrios' prohibitions.

2. The Peribolos of Dionysios (A3), possibly from Kollytos, contained an elaborate *naiskos*, a carved bull, and two guardian lions. It also witnessed a burial in the mid third century and the mid 2nd B.C.

3. The Peribolos of Lysimachides of Acharnai (A4) is not as elaborate as that of Dionysios, but contains a post Demetrian *kioniskos*, probably of Lysimachides' grandson.

4. Peribolos A15, while it appears not to have had elaborate statuary, is located in a privileged position and saw the Hellenistic addition of the name of Iatrokles to the fourth century *trapeza*.

5. Peribolos A19 contained a *trapeza*, two *cippi*, and four *kioniskoi*, one dated to the third century B.C.

6. The Peribolos of Eubios of Potamos (A21) contains a Doric column with a possible *loutrophoros* finial, a rosette stele, and most likely an additional relief stele. The length of burial extends at least three or four generations after 350 B.C.

The second point of similarity is post burial monument decoration, which would parallel the modern use of stone cleaning in Nisky Hill. Although we have no direct archaeological evidence of post 317 B.C. elaboration of the earlier tombs, our analysis of the Athenian custom of tomb visitation and commemoration of their dead demonstrates that these tombs must have received periodic decoration and must have been periodically visited after the law of Demetrios. As we have seen, the Greeks held frequent celebrations for their dead, and tomb decoration was a feature within this tradition. *Stelai* were often anointed with oil and decorated with a variety of objects ranging from the more unusual lyre and discus to the more common *taenia* or ribbon.[24]

In sum then, as a standing monument cemetery, the Kerameikos incorporates the same fundamental features as

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[23] The alphanumeric designation comes from R. Garland (1982) 'A first catalogue of Attic peribolos tombs', *Annual of the British School at Athens* 77 (1982) 125–76, which provides an excellent introductory bibliography for individual *periboloi*.

[24] See Garland (1985) 105–20 for overview.

witnessed in section E from Nisky Hill. Because standing monument cemeteries are contexts of tomb monuments, aggregated over time, they offer opportunities for status negotiation after burial. Topographical and cultural differences will shape the ways in which families might take advantage of the opportunities provided. In both contexts families did take advantage of the opportunity to bury in distinguished plots after sumptuary regulation. Yet, an important difference between the two cemeteries is that families in Bethlehem had the opportunity to duplicate existing elaborate markers, while the Demetrian legislation specifically excluded this option. Position is not such an important distinguishing variable in section E, because all monuments can readily be seen from the foot paths. But it was important in the Kerameikos, where tombs on the upper southern terrace were not easily seen from road level as one approached the city gate. Cultural differences also created further distinctions. People in Bethlehem do not have as extensive a tradition of tomb decoration as the ancient Athenians (although flowers are placed on some tombs at Easter and Memorial Day). For families with relatives in section E stone cleaning presents a more useful method to distinguish monuments after erection.

Perhaps the most important point brought out in this comparison however, is that, because the Kerameikos is also a standing monument cemetery, it has embedded in its internal structure the same contextual principle seen in Nisky Hill. Sumptuary legislation creates a distinction between those with already erected monuments and those now denied that opportunity. How does this principle then guide us in analyzing Athenian sumptuary laws? To answer this important question, we must introduce the full evidence for sumptuary legislation in ancient Greece.

THE EVIDENCE FOR FUNERAL LEGISLATION

Although the actual number of communities that legislated on funerary behavior is no doubt greater, historical sources indicate that there were at least 10 communities in ancient Greece that enacted some form of legislation that sought to control funerary behavior.[25] Athens, as usual, heads the list which is comprised of Catana (6th century B.C.), Delphi (ca.

[25] The most complete overview is Garland (1989).

400 B.C.), Gambreion (mid 3rd B.C.), Gortyn (600–525 B.C.), Iulis on Keos (450–400 B.C.), Myltilene (650–570 B.C.), Nisyrus (300–200 B.C.), Sparta (reportedly, 900–800 B.C.), and Syracuse (pre-Gelon, 490 B.C.).

Although the phenomenon of funerary legislation is more extensive than its Athenian appearance, my study will focus on the Athenian, because Athens presents the largest and best-studied corpus of funerary material in its excavations of the Kerameikos and Syndagma Square cemeteries. The case is however, problematical. There were three periods within which laws were enacted on funerary customs: that of Solon (c. 600 B.C.), that most likely dated to the early 5th century, and that of Demetrios of Phaleron (c. 317 B.C.). In each case however, our information is second-hand, coming not from inscriptional material (except in part for Demetrios) but from later writings of Demosthenes, Cicero, and Plutarch.

Let us look at the Solonian first. In his oration *Against Makartatos* (62), (341 B.C.) Demosthenes refers to earlier Solonian legislation.

> The deceased shall be laid out in the house any way one chooses, and they shall carry out the deceased on the day after that on which they lay him out, before the sun rises. And the men shall walk in front, when they carry him out, and the women behind. And no woman less than sixty years of age shall be permitted to enter the chamber of the deceased, or to follow the deceased when he is carried to the tomb, except those who are within the degree of children of cousins; nor shall any woman be permitted to enter the chamber of the deceased when the body is carried out, except those who are within the degree of children of cousins[26]

Plutarch (*Solon* XXI, 4–5), who must have been drawing on similar sources, relates Solonian prohibitions as well.

> Mourners tearing themselves to raise pity, and set wailing at one man's funeral to lament for another, he forbade. To offer an ox at the grave was not permitted, nor to bury above three pieces of dress with the body, or visit the tombs of any besides their own family, unless at the very funeral; . . .[27]

Since Solon, rules referring to funerary practices were an on-going concern for Athenians. Plato felt the need to discuss

[26] Translation from *Demosthenes: Private Orations* vol. II, trans. A.T. Murray (Cambridge MA: 1926) 103.

[27] Translation from *Plutarch: the Lives of the Noble Grecians and Romans*, trans. John Dryden (New York: 1932) 110.

such legislation in his *Laws* (xii.958d–960c), and it certainly appears that some sort of legislation was enacted in the early Classical period, although its exact date is uncertain. Cicero (*de Legibus* 11 26.64) is our only source for this legislation, often referred to as the 'post aliquando' law, but he is unfortunately, too brief.

> Some time later [i.e. after Solon] on account of the size of the tombs which we see in the Kerameikos, it was decreed that no one should make a tomb which required the work of more than ten men in three days, and that no tomb should be decorated with plaster (*opus tectorium*) or have the so-called 'herms' set on it.[28]

While the dating of this legislation remains difficult, such is not the case with that of Demetrios. According to the *Marmor Parium* (B.13) its passage was in 317/16 B.C. Cicero however, is our only full source for its contents.

> He, then, lessened extravagance not only by the provision of a penalty for it, but also by a rule in regard to the time of funerals; for he ordered that corpses should be buried before daybreak. But he also placed a limit upon newly erected monuments, providing that nothing should be built above the mound of earth except a small column no more than three cubits in height, or else a table or a small basin; and he put a special official in charge of the enforcement of these laws.[29]

In overview, changes in tomb display do appear to correlate with the legislation of Demetrios of Phaleron, and possibly with the legislation cited by Cicero, if it were enacted in the early Classical period. No such change is marked in the Solonian period, but it has to be remembered that our sources indicate that (to the extent that they are trustworthy) his prohibitions were not directed towards the construction of the funeral monument.

CURRENT THEORIES ON THE INTENT OF SUMPTUARY LEGISLATION

Attempting to explain exactly why some of these sumptuary laws appeared in Athens has been a tricky process. Temporal context is crucial here, since there must have been different

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[28] Translation from Kurtz and Boardman (1971) 90.

[29] Translation is from C.W. Keyes, *Cicero, De Republica, De Legibus* (Cambridge MA: 1953) 453–55.

social motives at different points in Athenian history. For example, Solon's legislation does differ from that of Demetrios, since it seeks to restrict mainly the funeral, while Demetrios sought to restrict the monuments. But most scholars see two overarching purposes for these laws: setting religious and social obligations for the care of the dead, and controlling display as part of a larger political strategy.[30] My interest lies here with the latter social strategy. Plutarch and subsequent authors have interpreted the Solonian legislation as a attempt to limit the opportunity for feuding between the families of Megakles and Kylon, which had begun by Megakles's assassination of Kylon.[31] The *post aliquando* legislation is somewhat different, and, with one exception, has been tied to the policies of either Kleisthenes or Themistokles, under the assumption that a decrease in ostentation in the cemetery was correlated to the production of democratic institutions.[32] Morris, however, rejects the idea of a single agent for this change and argues instead that the removal of ostentation in the cemetery was the result of larger internal leveling mechanisms.[33] Current views on the Demetrian legislation argue that its main purpose was to curb the financial burdens of rich Athenians or mollify the common's negative reaction to excessive display, or to emulate the restrained practices of his teacher, Theophrastus.[34]

The only person to offer an alternative explanation for these sumptuary laws in general has been Cannon, who is unique in

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[30] Garland (1989) provides a good overview, see also Morris (1992) 103–55.

[31] See Plutarch *Solon* 12; M. Alexiou (1974) *The Ritual Lament in Greek Tradition* (Cambridge: 1974) 21; and Garland (1989) 4–5.

[32] This correlation has a long history. Its most recent adherents are F. Eckstein (1958) 'Die Attischen Grabmaelergesetze' *Jahrbuch des deutschen archaeologischen Instituts* 73 (1958) 18–29; C. Clairmont (1970) *Gravestone and Epigram* (Mainz: 1971); V. Zinserling (1965) *Wissenschftliche Zeitschrift der Freidrich-Schiller-Universitaet Jena* (1965) 29–31. There have been some exceptions however. See G.M.A. Richter (1961) *The Archaic Gravestones of Attica* (London: 1961), and Kurtz and Boardman (1971) 90.

[33] Morris (1992) 128–55.

[34] The view the Demetrios was somehow trying to lift the burdens of funeral expense, (the elite were caught in a hopeless game of display in the cemeteries), and civic liturgies, was advanced early on by W.S. Ferguson (1911) *Hellenistic Athens* (London: 1911) 42; and later by Garland (1989) 8. Others have seen different motivations for Demetrios' action. Kurtz and Boardman (1971) 166, see his sumptuary legislation as stemming from his education at the feet of the philosopher Theophrastus, who apparently had a very restrained funeral. C. Mossee (1973) *Athens in Decline 404–86 B.C.* (London and Boston: 1973) 105, would see his measures as attempts to appease the Athenian commons, who were reacting to the elite excesses of the earlier fourth century.

that he does not come from a Classical background. He argues that there is a general principle for change in mortuary display that affects all historical configurations. That principle is that a move toward less ostentatious graves and funeral display is not a strategy on the part of the nonelites, but on the part of the elites, who switch the social markers of hierarchy from those of ostentation to those of nonelaboration, in order to maintain a social distinction, when ostentation has become redundant.[35]

The Texts in the Shadow of the Monuments

Past explanations for the reduction of ostentation within Athens' standing monument cemeteries suffer from an incorrect interpretation of the full character of Athens' standing monument cemeteries. Whether one follows arguments that argue for the controlling hand of the commons or that of the elite, one is still making a serious error because both view the cemetery in a limited synchronic perspective, focusing on only the monuments that would be erected at the time of the legislation. They assume that society would focus only on current grave markers, ignoring the important shadow cast by the monuments which have already been erected. They deny the identification of Athenian cemeteries as standing monument types whose full contextual character must be included in any attempt to understand the social meaning of laws that affect it.

Reference to the embedded contextual principle by which standing monument cemeteries are affected by sumptuary legislation can provide analytical frames for a search for larger social meaning behind their correlated sumptuary legislation.[36] In this I am not trying to apply any law-like generalization to

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[35] Cannon (1989).

[36] I take the position that those who enacted the funeral legislation were aware of the operational features of their own cemeteries. There is a good case to be made that Athenians did understand the unique operational features of their cemeteries. First, Athenians were apparently familiar with Solon's legislation, enough for it to be understood as a referent in court cases, and must have recognized his attempt to curb this type of behavior. Plutarch's reference to Solon does show that he did attempt to restrict the secondary use of the cemetery for status display, by ruling that mourners were not allowed to visit the tombs of any besides their own family, unless at the very funeral. Second, other communities were certainly aware of this same operational feature. Several communities passed mortuary sumptuary legislation that prohibited post burial status negotiation. Delphi prohibited commemoration at any other grave than that of the deceased during

the study of ancient Athenian society. The method here does not compare to that of Cannon, who in constructing his principle argued that restriction in funeral monuments was the result of an ahistorical process of elite control of cemetery display, seen in a timeless elite lead and lower class emulation of display.[37] As Morris correctly pointed out, the assumption that change within a cemetery will always correlate to elite control, requires a certain leap of faith.[38] The method I am using is one that has isolated an inherent *contextual* principle that is incorporated within the context of a standing monument cemetery. This feature does not as part of its definition correlate with any specific group within the community. The cemetery in this view operates in a fashion similar to any artifact that has cross-cultural operational features, but can easily be used in a variety of different ways by different cultures and groups with cultures, whether they be elite or otherwise.

I would now like to turn to two cases and discuss how an analytical frame based upon the principle that a standing monument cemetery provides a context for established—non-established distinction under the effect of sumptuary legislation, can be used. The first is the early Classical period, when the 'post aliquando' legislation was most likely enacted. There is little left *in situ* of the Kerameikos in the period from c. 530–480 B.C. Much of the monumental data was built into the Themistoklean (c. 478–477 B.C.) wall. But even though this is so, we may, with some measure of surety, assume that before the razing of these stones, the Kerameikos was a cemetery that was punctuated with many elaborate monuments from the late Archaic period that stood out in contrast to the newer small Classical *stelai*. In a real sense, the structure of this context must have been very much the same as that of the Demetrian period, and the operational distinctions of a standing monument cemetery must have been present.

The 'post aliquando' legislation which most likely dates to this period and restricts monumental display, would then have created a distinction between families with ostentatious stones and new families without. The legislation was not therefore

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the funeral; Gambreion tried to limit the length of time within which it would be appropriate to commemorate the dead after burial; and Iulis legislated to abolish the thirtieth-day celebration. Can the Athenians have been so isolated that they differed in their knowledge of these features?

[37] Cannon (1989).

[38] Morris's reply in Cannon 1989, 451–52; Morris 1992, 147–48.

intended to curb elite excesses and promote an egalitarian ethos as a step towards democracy as proposed by ancient historians, it was intended to deny the Kerameikos as a context for status negotiation to those who had not used it as such yet. The context of commemoration of the dead was thus removed as an option to new families for status negotiation. This interpretation demonstrates that we cannot simply flag the legislation as a hallmark of early democracy in Athens. The possible correlations that restrictive legislation had with social motives were much more complex. It would be much more profitable to ask who might both the established families and the new families have been.

Such an investigation is not in the scope of this essay, but I would argue that, in light of my discussion, it is an investigation that must be sensitive to various interpretations, rather than current elite/commoner distinctions.[39] To take but one, if we were to assume that Cannon's argument, that fashion in stone design was begun by elites and quickly followed by groups under them, were applicable in this one instance, we would have to consider that the elaborate Archaic stones represented a heterogeneous group of older elite families and others who would have emulated their ostentation. This presents a possibility that the Archaic monuments could correlate with the more aristocratic Athenian elites, newer elites from non-aristocratic backgrounds, and non-elite groups who would want to use the cemetery context for display of status aspiration, as has been proposed for twentieth-century English cemeteries.[40] On the other side of the equation, we should shy away from identifying those who would have emulated this ostentatious fashion, and were now denied the opportunity, as commoners. This was also probably a mixed group, ranging from families with new wealth and power that wanted to legitimate this status in the cemetery to those who were wealthy, like the metics, but denied full franchise within Athens.[41] It becomes the task of both the historian and archaeologist to determine what the possible compositions might be and who within these hetero-

[39] Morris 1992, 103–55.

[40] M. Parker Pearson (1982) 'Mortuary Practices, Society and Ideology: an Ethnoarchaeological Case Study', in *Symbolic and Structural Archaeology* ed. I. Hodder (Cambridge, 1982) 99–113.

[41] Recent overviews have shown that the Kerameikos monuments represent a wide spectrum of people within the community, from elite to poor to slaves to metics. For a stimulating recent discussion see T.H. Nielson, L.

geneous groups might benefit from new limitations on social definition within this context at this specific point in Athenian social history.[42]

While I have just focused on a period of Athenian history that is crucial to the understanding of the democracy of the fifth and fourth centuries, I could also have applied this analysis to the period of Demetrios of Phaleron. To repeat, current explanations view the actions of Demetrios as basically cutting the financial burdens of the elites, philosophically inspired, or designed to calm the commons' objections to elite excess. Again, however, if we apply our frame for analysis—the expectation that sumptuary legislation would draw a distinct line between those who have used the cemetery for status negotiation and those who are now denied the opportunity—we are using an approach that could better correlate the changes in the cemetery with some other changes that Demetrios made in Athenian society.

Even though Demetrios expanded the limited franchise that was imposed on Athens by Kassander, there is no doubt that he was trying to establish some type of oligarchy. An oligarchy almost by its very definition, depends upon a stabilization of social position, and is threatened by social mobility—new individuals with financial means, but no political position are an obvious threat. Thus, Demetrios' attempt to curb monumentality in the cemeteries can be seen as an attempt to freeze the current social order, denying the use of the cemetery as a context of status negotiation to emergent groups. This conclusion correlates well with Demetrios' removing the dramatic festival and other associated civic liturgies from the elites and funding them with state money. The charge that subventing these civic affairs was an unfair burden on the elites is a popular elite response to such expenditure, which should not be taken as a universal sympathy in Athens.[43] What is more likely is that Demetrios saw the civic festivals as a dangerous means by

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Bjertrup, M.H. Hansen, L. Rubinstein, T. Vestergaard (1989) 'Athenian Grave Monuments and Social Class', *Greek, Roman and Byzantine Studies* 30 (1989) 411–20.

[42] I am not suggesting here that we follow Morris' (1987) lead and see elites and commoners distinguished by exclusion from burial within formal cemeteries. The case is far more sensitive than this opposition would mark.

[43] This explanation for Demetrios' removal of these liturgies from the elites has been offered by Ferguson (1911) 55–57.

which marginalized and emergent individuals could accrue Athenian popular support and thereby threaten the order.

ARCHAEOLOGICAL INDEPENDENCE AND ATHENIAN SOCIAL RECONSTRUCTION

Why have the basic distinctions and a very important principle for Athens' cemeteries been overlooked by scholars of ancient Greece? The answer lies in their approach to the archaeological material. As I stated in my introduction to this book, most scholars of ancient Greece subordinate the archaeological record to the textual in the belief that only historical data can provide a model for social structure. Archaeological evidence is held to be capable of only supplying assistance, most often filling in the gaps and outlining daily life. But such thinking denies the possibility that the archaeological record can offer independent evidence that, taken with the textual, can produce significant insights into past societies.

In this argument I side with Leone, Potter and Crosby; and Kosso (this volume), who argue strongly for the benefits of analyzing both the archaeological and textual record as independent evidence that can make itself amenable to middle range theorizing.[44] As they describe it, we should be able to use the documentary record to create a grid against which we can compare the archaeological. Aspects of this record that do not meet expectations generated from the textual are said to provide an ambiguity, the analysis of which provides the main research goal for the archaeologist.

To demonstrate my point I would like to examine the currently accepted approach to the reconstruction of Athenian social structure from the study of mortuary material. In his recent work, *Death-ritual and ancient Social Structure*, Morris, in good historical method, draws upon ancient authorities, such as Thucydides, Aristophanes, Demosthenes, and recent historical analyses of these works by Ober and Loraux, to argue that the fifth century in Athens framed a shift from an emphasis on

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[44] M. Leone and P. Potter Jr. (1989) 'Issues in Historical Archaeology', in *The Recovery of Meaning: Historical Archaeology in the Eastern United States* ed. M. Leone and P. Potter Jr. (Washington: 1989) 1–23; M. Leone and C. Crosby (1987) 'Epilogue: Middle-Range Theory in Historical Archaeology', in *Consumer Choice in Historical Archaeology*, ed. S. Spencer-Wood (New York: 1987) 397–410.

the individual to one on the group, or in his case, the polis.[45] Accordingly, he gives meaning to the apparent restraint on material items of wealth in both the cemetery and domestic contexts, as the result of a social structure in which '... the mass of citizens defined the evaluation of material culture.'[46] Thus, the use of ostentatious materials in Athenian society was made problematic for elites, by giving such items non-democratic values that did not correlate with prevailing egalitarian and communal ideals. With this textually-constructed element of social structure Morris sees the Athenian mortuary record as dependent evidence, supporting the textually-based social reconstruction by offering a frame for dynamic analysis. '... they (burials) can be used to *augment* (emphasis mine) the written record, giving us for the first time a dynamic account of social structure and how it changed in antiquity.'[47]

But this dependent relationship between textual and archaeological evidence in this case spawns a methodology that produces a very distorted picture of Athenian social structure. Let me look at two specific problems. First, an assumption that archaeology provides a dynamic for textual structure has led to a myopic focus on changes within the burial context and not the context itself. Additionally, evidence produced by interment and that produced by behavior after burial has been conflated. The contexts of each are only weakly linked, and then only by chronological sequence. The funeral celebration which produced the grave goods did take place within the cemetery, but the cemetery as a separate context continued to operate in a different fashion afterwards. Dynamic patterns in each context were dependent on different contextual distinctions and should not be assumed to be the result of the same social strategies.

Secondly, and perhaps most crucially, important social information that can be supplied by Athenian cemeteries, information which is just as independent as textual, is overlooked

[45] I would to make it clear that I am in no way singling out Morris' work as insignificant. He has done much to bring the study of mortuary material to ancient history. His research paradigm is grounded in ancient history and, as an archaeologist I take issue with this method in the study of ancient Athens. For his sources see Thucydides 2.35–46; Aristophanes (*Knights* 1321–34; *Clouds* 984–86); Demosthenes 23.207–78; 3.2526; 13.20; J. Ober (1989) *Mass and Elite in Democratic Athens* (Princeton, 1989); N. Loraux (1987) *The Invention of Athens* (Cambridge MA, 1987).

[46] Morris (1992), 152.

[47] Morris (1992), 1.

in social reconstruction. While I agree that the contexts of the assembly, court, state funeral, el al. that produced the texts from which ancient historians recreate social structure, did de-emphasize previous elite values, I would not automatically assume that other archaeological contexts can supply testing grounds for the dynamic operations of this structural element. My analysis of the Athenian standing monument cemetery demonstrates that expectations coming from this reconstructed social structure, are not met in the cemetery context. Instead, what a comparison of this textually generated social structure and the mortuary archaeological record really shows, is not a dynamic of commons-generated restraint in display placed upon elites, but something much more subtle, a shift in the opportunities available to groups to use cemeteries in Athens for social negotiation. Yet, this shift in no way correlates with a complete denial of elite use of the cemetery.

As Leone, Potter and Crosby would observe, herein lies the ambiguity. The cemeteries were not contexts that were similar to the assembly, court, theater, et al. in their social restrictions. Elites could use the cemeteries for status negotiation. This disjunction of contexts within ancient Athenian society is *the* crucial issue that needs to be addressed in the reconstruction of social structure, not an assumed dynamic mapping of documented restraint seen in other contexts. Equating textual and archaeological material demonstrates that, to be valid, a model of Athenian social structure has to be one that can incorporate several contexts, which might be very much different in their internal hierarchy, into a unique complex portrait.

CONCLUSION

Ancient Greece contains one of the first cases of standing monument cemeteries in the world. Embedded within the internal structure of these contexts was a principle that restrictions in ostentation placed upon an existing cemetery created a distinction between established and yet-to-be established families and individuals in respect to monument size and elaboration. Scholars of ancient Athens, like their archaeological counterparts in other regions of study, have thrown these cemeteries into an analytical bin with other types of cemeteries and have failed to realize the importance of their independence and distinctiveness.

At the root of the problem lies the pervasive assumption

that archaeological material must stand as secondary support to textual, which, in its apparent greater depth supplies cultural models. If there is any lesson to be gained from this essay, it is that such assumptions are dangerous. Ancient Athens and its preserved cemeteries does provide an excellent context for the study of Greek social change, but only if we rethink current methodological positions and open our eyes to the full power of the archaeological record.

CROSS-CULTURAL VIEWS

EPISTEMIC INDEPENDENCE BETWEEN TEXTUAL AND MATERIAL EVIDENCE[1]

PETER KOSSO

Claims about the human past require some form of evidence, whether textual or archaeological, to make the case for accuracy and credibility. The epistemic work is done most effectively by independent evidence, that is, evidence that is not itself influenced by or in collusion with the claim it is supposed to support. Many archaeologists and historians have been explicit in emphasizing this kind of independence. Binford, for example, in explanation of the benefits of middle range research in archaeology, links the objectivity of theory-testing to, 'the status of logical or intellectual independence between ideas being evaluated'.[2] And historical archaeology is particularly suited to exploit the benefits of independence as between textual and material sources of evidence. This has been openly advocated by Leone and Crosby, and by Leone and Potter.[3]

The concept of independence which is at work in these cases is invariably left unanalyzed, but given the heavy epistemic burden it carries, independence deserves some attention itself. A close look will show that there is in fact a variety of ways that claims about the past can be independent of one another. There are, in other words, several kinds of independence between such claims, whether between two evidential claims or between an evidential claim and the reconstruction of the past it is meant to test, and it will be important to see which of these kinds of independence is up to the epistemic task.

Once the specifically epistemic form of independence has

[1] My work has been supported by a grant (DIR-8917989) from the National Science Foundation.

[2] L. Binford (1982) 'Objectivity-Explanation-Archaeology 1981', in *Theory and Explanation in Archaeology* eds. C. Renfrew, M. Rowlands, and B. Segraves (New York: 1982) 128.

[3] M. Leone and C. Crosby (1987) 'Middle-Range Theory in Historical Archaeology', in *Consumer Choice in Historical Archaelogy* ed. S. Spencer-Wood (New York 1987) 397–410; M. Leone and P. Potter (1988) 'Introduction: Issues in Historical Archaeology', in *The Recovery of Meaning* eds. M. Leone and P. Potter (Washington: 1988) 1–22.

been identified it will be seen to cut across the distinction between text and artifact or the disciplinary divide between history and archaeology. Evidence from the textual record is not necessarily independent, in the relevant epistemic sense, from evidence from the material record. So while texts and archaeology are often a good source of independent support for each other, this independence cannot be assumed.

Accountable Evidence

Evidential claims, if they are to be useful in testing hypotheses about objects and events outside of immediate experience, must themselves make reference to things in the same, unexperienced realm. Whether it is of the past, the very small, or the very distant, evidence is relevant to theory only when it speaks of events that touch the objects of theory. Evidence reports the connection between what can be observed and what cannot, and for this reason, evidential claims are themselves in need of some support, some justification and account of the reliability of the link between what is manifest in experience and what is of interest. This account is most naturally provided through a description of how the data were formed. This is true of the textual record, where knowing the intentions, abilities, and activities of the author helps in assessing the reliability of the written description. It is true as well of the material record, where, as Binford and Schiffer have made clear, an understanding of deposition and subsequent alteration of artifacts is necessary for dealing with the 'distorted reflection of a past behavior' presented by the material evidence.[4]

This sort of accounting of the evidence contributes two things: it provides justification for evidential claims and it enhances the meaning of the evidence. There are two senses of 'meaning' with reference to evidence of the past, though both pertain to what the evidence is *of*. In one sense the meaning of evidence is its physical indication, as in 'these clouds mean rain this afternoon,' or, 'these wasters mean there was a ceramic workshop and kiln at this site.' The wasters are evidence of the kiln, but this is known only with the assistance of some, in this case pretty minimal, background claims. The background claims in this example are about the behavior of ceramic workers and the properties of clay. These describe the link between

[4] M. Schiffer (1976) *Behavioral Archaeology* (New York: 1976) 12.

what the evidence means, the kiln and associated activity, and what is seen, blobs of ceramic. They play the role of middle range theory by answering the question, 'what meaning is to be attributed to empirical observations?'.[5]

The other sense of meaning applies to the intentional reference of words and symbols. For example, a bit of textual evidence of Athenian cleruchies is secured with the realization that Thucydides means a cleruchy when he describes a colony (anoikoi) at Mytilini.[6] This meaning is understood from context and a knowledge of other circumstances of the time and place. The meaning of the Thucydidean passage, its evidential content, is secured by accounting claims that speak of other objects and events in the past.

The same sort of accounting claims provide the justification of evidence and secure the reliability and accuracy of evidence. Such justification is an epistemic responsibility, as the informational link, from pieces of ceramic to their being wasters to the presence of a kiln, is by no means indubitable. The link and the justification are found in the description of the source, in this case the kiln, and the events through which the information is transferred. Such accounting is well known in the context of archaeology as it is the business of middle range analysis. The textual record bears the same epistemic responsibility of justification, and in this sense history, no less than archaeology, calls for and benefits from middle range research. In the historical context the accounting claims, the response to questions of credibility of a text, include claims of the intentions of the author, the author's access to the events and his, or his witness' qualifications as an observer. A full appreciation of both the meaning and reliability of textual information requires some understanding of the author's presuppositions and the conceptual context of the writing, as well as more mundane matters of the treatment of the text over time and the reliability of its transcription and translation.

The point so far is simply this. No evidence for claims about the past is above the responsibility of justification. Nor is its informational content, its meaning in either sense, obvious or immune from ambiguity. Fulfilling both requirements, justification and informational content, is the role of accounting

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[5] L. Binford (1977) 'General Introduction', in *For Theory Building in Archaeology*, ed. L. Binford (New York: 1977) 2.

[6] A. Graham (1983) *Colony and Mother City in Ancient Greece* (Manchester: 1983) 170.

claims, and only such accountable evidence is acceptable evidence. Furthermore, and this is important, the accounting claims are also, at least in part, about objects and events in the past and are themselves in need of justification.

In the usual case the accounting claims do not have any epistemic priority over either the evidential claims they support or the original reconstruction of the past the evidence is intended to test. The relevance and reliability of the evidence, in other words, is not inherited from an antecedent relevance and reliability of the accounting claims. The accounting claims are generally no more objective, empirical, or whatever is the appropriate epistemological standard, than are the evidential claims they serve to justify. The epistemic authority does not come from one side in the intersection of accountants and evidence; it comes instead from the feature of independence between the accounting claims and the beneficiary of the evidence.

Philosophers of science are fond of the slogan that evidence is theory-laden, and they have in mind exactly the sort of ladening with accounting claims as described above for evidence in archaeology and history. But the influence of theory on evidence does not lead to a problematic circularity in the testing of theories provided that the observation is laden only by theories which are independent of the particular theory for which the observation is evidence. An insistence on independence blocks any self-serving testing of a theory by evidence of its own sponsorship. In this way it is acknowledged that testing and the evaluation of our claims about the world must be done from within our own conceptual position, but it can be done at least outside of the influence of the particular theory under evaluation. Independence, in this sense, is the key to objective justification.

This analysis applies to studies of the human past no less than to natural science, and it suggests an opportunity for history and archaeology to cooperate. Texts and artifacts can be sought as independent sources of information to serve as accounting claims, as middle range theories, for each other. Leone and Potter are explicit in advocating the middle range strategy in historical archaeology by the use of texts and archaeology as independent evidence. Thus, 'four parts of middle-range theory are particularly useful for historical archaeology: (1) the independence of the archaeological and the documentary records, (2) the concept of ambiguity, (3) the use of de-

scriptive grids, and (4) the idea of organizational behavior'.[7] David Small also takes advantage of the epistemic value of independence, pointing out that 'texts often supply enough detail for archaeologists to impute the structure behind independent data'.[8] Neither source of information, texts or archaeology, is assumed to have more epistemic authority than the other. One is useful, persuasive evidence for the other because of their mutual independence.

It will be helpful to be clear on exactly why independence is epistemically beneficial and to articulate the intuition behind the methodology. A large part of the concern in testing and judging claims about the past is the prevention of systematic errors as would be caused, for example, by a consistently mistaken dating process or a persuasive but deceptive author. Relying on the likes of this evidence would be like viewing the world through distorting glasses in that it would consistently affect every view and every piece of evidence acquired through those means. The distortions will not be revealed by looking at more things or by looking more closely at the same things. More looking is not the answer; using other senses, for example feeling the shapes we see or feeling the heat of the fire we see, is the more informative evidence. But it is not more informative because feeling is generally more reliable or more accurate than seeing. It is rather that feeling is an independent source of information and the chance that such independent sources will report the same falsehood or will coincidentally support each other's reports is small, smaller at least than the chance of any one of them being false.

An analogy helps to further understand this intuition and to point out the features of the particularly epistemic kind of independence. In courts of law, the jury and officers of the court must be aware that sometimes witness lie, misremember, and misperceive, not unlike historical sources. Credibility of a witness can be based on several factors including a consistency of their testimony with information from the material evidence. If the witness says he heard two shots, and two bullets from the scene are produced as evidence, there is reason to believe the testimony. The key to this justification is that the material evidence is independent in the sense that the witness cannot

[7] Leone and Potter (1988) 12–13.

[8] D. Small (1987) 'Toward a Competent Structuralist Archaeology: A contribution from Historical Studies', *Journal of Anthropological Archaeology* 6 (1987) 107.

tamper with it and, in the best case, has no previous knowledge of it, and so is powerless to concoct a consistent story. Where there is no collusion, consistency is more likely an indication of truth. It works as well to compare one witness' account with that of another, independent, witness. Most important is to hear testimony from witnesses with no close relation to the defendant and nothing to gain from the outcome of the trial. Consistency in the accounts from two witnesses with close ties or with a vested interest in the verdict is not so impressive since it is little more than a single story retold. A second hearing is no serious test of the first.

The analogy between a court trial and studies of the human past can be very informative. The relationship between texts and archaeology is similar to the use of testimony and material evidence in that either can be used as independent sources of information to function in the accountability of the other as evidence. There are two distinct ways to use independent claims as accounting of evidence. Two claims can provide separate reports on the same or related events, on the one hand, or one claim can describe part of the medium of information of the other claim.

The first opportunity for independence is the more familiar. An example would be Pausanius' accounts of the terrain and monuments of Greece, accounts which can be compared to evidence in the material record as it is seen today. In this case, the written record and the material remains are of the same objects. For another example, there is evidence as provided by Herodotus about the forces of Xerxes in the Persian war. There is also an inscribed list with the same information found at the site of a Persian camp, so again the two sources of information report the same event.[9] It is rare though for texts and archaeology to report on the same object or event. This is due to the difference in focus between the textual and material records, and the difference of survival of texts and artifacts. Most importantly, texts and artifacts tend to report on different *kinds* of things, as Snodgrass describes. 'the general nature of archaeological evidence: it seldom speaks the language of historical events'.[10] While texts are most often about short-term events of punctual significance, events such as battles,

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[9] D. Lewis (1985) 'Persians in Herodotus', in *The Greek Historians* (Stanford: 1985) 103–104.

[10] A. Snodgrass (1983) 'Archaeology', in *Sources for Ancient History* ed. M. Crawford (Cambridge: 1983) 166.

treaties, and expeditions, the archaeological evidence records more long-term processes and mundane behavior such as farming, the building of roads, and the habitation of buildings. Thus the two sources of evidence are rarely about the *same* events of objects, but they are often about *related* things. For example, the textual, historical account of the establishing of an Athenian cleruchy would be supported by the related material evidence of an organized network of farms. The separate channels of evidence would produce a coherent picture, assembled from independently supplied pieces. Such coherence without collusion would be a mark of epistemic justification. It is what sets factual studies of the past apart from historical novels which offer the reader coherence by design.

The other opportunity for independence involves one claim which is relevant to the description of the conveyance of information contained in another evidential claim. The clearest examples of this are in archaeology where other disciplines are used in the role of middle range theories for archaeological evidence. Dating, for example, by radiometric or dendrochronological means, requires substantial support from an understanding of radioactivity or botany, things that have nothing to do with the objects of archaeological interest, the past behavior. Archaeological evidence is also influenced by claims about the environmental effects on materials, erosion, for example, or the development of petina, claims that are relevant to the formation of the evidence but not to the object of the evidence.

There is opportunity to use this mode of independence in the meeting of texts and archaeology as well. When potsherds are dated by their matching comparanda, themselves sherds often found in context with dated texts, the texts are being used to both enhance the meaning of the original sherd and to secure the reliability of the information on dating. Even though the texts themselves are not about the object of material evidence, they are independent participants in the accountability of that evidence. As another example, Ian Morris skillfully uses burial remains as evidence of the social status of the persons buried. The informational link between social status and the physical characteristics of burial is supported by the 'unintended evidence' in literature showing presumptions and implicit attitudes toward funerary rights.[11] The textual information is

[11] I. Morris (1987) *Burial and Ancient Society* (Cambridge: 1987) 44.

relevant to the link between observation (of burial remains) and interest (social structure). Yet a third occurrence of this use of independence is specific to the case of inscriptions, of text-as-artifact. In this case the provenience of the text can be indicative of the location, circumstances, and even the motive of its writing. This contextual information, though not about the subject of the text, can contribute to an understanding of both its meaning and its reliability.

Having outlined two ways that independent claims can be used to epistemic advantage, we are in a position to see that not just any manner of difference between claims is sufficient to do the epistemic work. Insofar as there is a variety of ways that evidential claims can differ, there is a variety of kinds of independence. In the next section, the particular style of independence which is epistemically significant will be identified. From the characteristics of this epistemic independence it will be seen that the distinction it draws between independent claims does not match the distinction between textual and material evidence or between history and archaeology. Using these two kinds of evidence does not guarantee independence.

Varieties of Independence

The differences between textual and material evidence that are to be articulated below are largely generalizable to all sorts of claims that report on what is not manifest in experience. Individual claims about the past, the very small, or the very distant, bear a kind of independence from each other if they differ in the following ways.

Two evidential claims could differ in that they result from different types of informational links between the observable record and the object of evidential interest. Texts and archaeology are a good example of this kind of independence. Textual information is generally initiated by an act of selection by an author. Textual evidence is the product of intentional communication through a medium of meaningful symbols with make direct reference to the objects and events of interest. The crucial steps of informational transmission in the textual case are episodes of purposeful, human interaction. That is, the style of informational link which supports textual evidence. The information in material evidence, by contrast, depends on a selection process that begins with a largely unintentional deposition and a subsequent natural selection for those materials

which endure the environmental degradation. The information in this case is passed and altered through a channel linked by both human and natural interactions, and differs significantly from the textual signal in the diminished role of human intentions. The two types of evidence, in other words, differ in terms of the way the information is made available.

For future reference, call this manner of difference between evidential claims *transmission-type independence*, since the information is transmitted by different types of interaction between the observer and the objects of evidential interest. We will put off for a moment the crucial question of the relevance of this kind of independence to the epistemic task. It will be important to know whether transmission-type independence is necessary, or sufficient, or perhaps only loosely indicative of the epistemic independence which is the key to justification of evidence.

Note first that this sort of independence is often what people have in mind when appealing to texts and archaeology as independent evidence. Leone and Crosby, for example, point out the epistemic advantage to using texts and archaeology in complementary middle range research. 'Our two sources of data—the archaeological and the documentary—are generated by two very different sets of formation processes and dynamics, and therefore two very different sorts of facts are generated'.[12]

Before asking whether this sort of difference is an epistemically significant difference, it will be helpful to note some other ways that data and evidential claims can be dissimilar.

Two evidential claims could differ by being formed through separate tokens of informational link, that is, by distinct, though not necessarily of a different type, interactions with the objects. Again, just for future reference, call this *transmission-token independence.* The previous distinction is a subset of this one in that any pair of claims which are transmission-type independent are also transmission-token independent. The converse is not true. Two pieces of textual evidence, for example, that are written by different authors will be transmission-token independent but not transmission-type independent since the channels are distinct but of the same kind. There are similar examples within archeology where two evidential claims are based on separate, though of the same type of artifacts of separate but similar sites. And of course there are examples of

[12] Leone and Crosby (1987) 399.

evidential claims that are both transmission-token and transmission-type independent. Any pairing of textual and archaeological evidence falls into this category.

Transmission-token independence comes in degrees. Different sources of information constitute transmission-token independence and this is a difference that can vary. Separate artifacts found in close proximity will produce evidence with less transmission independence than artifacts found in different rooms, and less still than those from separate sites and even distant regions. The measure of independence that results from this could be used as a scaling of justification in the sense that help from more independent evidence adds more credibility than help from less independent. But before the analysis can deal with details of this sort we must ask generally of the epistemic significance of transmission-token independence.

Transmission-token independence, like its more specialized subset transmission-type independence, matches some instances of current use of the concept of evidential independence. Again Leone and Crosby focus on the independence of the data, noting that, 'The data were made by different individuals, at different times, for different purposes'.[13] This is an independence which keys not so much on different types of informational processes but on different individuals, and hence different tokens, of informational transfer.

There is another kind of independence that is possible between evidential claims: they can differ in terms of what they are about, that is, what they refer to. If two claims refer to distinct objects or events, they are *reference-token independent.* This is a consideration of the meaning of the evidence rather than its formation process. A few simple examples will help to clarify this kind of independence.

Consider two examples of archaeological evidence. A light scatter of potsherds in areas close to, but not on, a particular site in southern Euboea may be used as evidence of cultivation of the nearby land and derivatively as evidence that the site was used for farming. A separate set of evidence from a similar but distinct site in Attica may serve as evidence about that site, that it too is a farm. Insofar as one evidential claim is of a Euboean site and the other is of an Attic site, these claims refer to different individuals and are reference-token independent, even though they refer to objects of the same type.

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[13] Leone and Crosby (1987) 399.

As another, related, example, the Athenian Tribute Lists, which are about taxes owed to Athens, can be used as evidence for instances of Athenian colonization since an abrupt reduction in tribute is often associated with the seizure of land by Athenians and a resulting loss of tax burden for the displaced people. There is this kind of evidence in the Athenian Tribute Lists for the imposition of an Athenian cleruchy at Karystos in 449 BC. The tribute reduction is from 7 1/2 talents in 450 to 5 talents in 449. Separate evidence, from archaeological survey of an area very near Karystos, shows an orderly complex of farmsites, tentatively dated to the late 5th century BC, an orderly complex as would expected of a settlement established all at once, a settlement such as a cleruchy.[14] Thus the one piece of evidence about taxes, and the other about a settlement pattern, refer to different things and are in this sense reference-token independent. They can provide independent pieces of what seems to be a coherent picture of the past, a picture in this case of an Athenian cleruchy.

Again though, we will have to be careful as to whether this kind of independence is sufficient or even necessary to do the epistemic work.

The example of the Athenian Tribute List evidence and the farm-complex evidence are of claims that refer to different things. They are, in this case, not only different individuals, different tokens, but they are in fact different types of things. One piece of evidence refers to taxes and economic behavior while the other is of settlement patterns and agricultural activities. For this reason it is also an example of *reference-type independence.* Evidential claims which refer to distinct types of things are reference-type independent, a subset of the previous reference-token independence. This type of independence, like the transmission-type independence, nicely matches important aspects of the distinction between texts and archaeology. That is, not only do these kinds of sources differ in terms of how the information is conveyed, they differ as to what kinds of things they are generally about. Historical evidence, texts, are most often about events and persons of punctuated importance such as battles, treaties, political authorities and their decisions. Texts generally inform us of the elite and powerful members of a culture, focusing on the political and social aspects of their

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[14] D. Keller and M. Wallace (1986) 'The Canadian Karystia Project', *Echoes Du Monde Classique/Classical Views* 30 n.s.5 (1986) 155–59.

behavior. Archaeology, on the other hand, tends to be informative of the common and disenfranchised members of a culture, more informative, at least, than is history. Archaeological evidence is of long-term, day-to-day processes of the past, with a focus on the functional and cultural aspects of behavior.

With these differences of emphasis and perspectives, differences in the type of things referred to, textual and archaeological evidence show a kind of independence such that when they agree, when they add up to a coherent account, it seems to be more than a chance coincidence.

There are, of course, a lot of trivial differences between evidential claims of the past, trivial in that they obviously have no epistemic significance. Claims differ, for example, as to when they were proposed or discovered. They differ in terms of who first introduced them and in which journals. These are clearly not the sorts of differences which amount to the independence between claims which makes their agreement or cooperation an indication of truth. It is interesting though to contrast these unimportant differences with the one which most explicitly gets to the epistemological issue.

Evidential claims about the past can differ in terms of the reasons we have to believe them, that is, in terms of the basis of their justification. Regardless of their reference or the type of informational link to the past, evidential claims require justification and this can be shared or separate between any two tokens of evidence. When the justification is separate, two claims show an explicitly *epistemic independence*. This can be made more precise in the following way. A claim x is epistemically independent of y just in case y does not entail any of the justification claims used to support x. Thus, if y does not contribute to the credibility of x, x can be used as independent evidence for y without incurring the problematic circularity of x supporting y while y supports x. When there is epistemic independence, x must be supported by separate means.

This kind of independence directly accomplishes the intended benefits originally set out for independent evidence. If evidential claims are independent in this way, they present meaningful evidence, in the sense that claims to be tested take genuine risks in confronting the data, because the testing process is not circular. Furthermore, independent evidence in this sense avoids the systematic errors which would be missed in relying on a single piece of evidence or even several in collusion. The point is that there are no solid, unquestionable claims or evi-

dence when knowledge is extended beyond manifest experience. If ever evidential claims need justification, then it is better to spread justification broadly for a coherence among other claims rather than to rely on a single point of support. Epistemic independence is an indication of this spread in that it shows the diversity of support for separate evidential claims.

Epistemic independence can be evaluated with the information available in the present. It is based on relationships of implication and explanatory relevance among statements; that is, among the things that investigators in the present have to work with. Contrast this with transmission independence, type or token, whose evaluation depends on additional expansive claims, claims about events in the past, describing the informational link between the present record and the past. This evaluation, in other words, depends on information which is not directly accessible to us and so adjudication of physical independence raises further epistemological questions.

In sum, epistemic independence as described above is both amenable to evaluation and effective as a hallmark of good, credible evidence. Epistemic independence is what we want as the distinction of independent evidence. Some of the other kinds of independence though may well be related to the epistemic independence as reliable symptoms or at least general indicators. The relationship among the varieties of independence is then the next important issue.

Texts, Archaeology and Epistemic Independence

As pointed out earlier, transmission-type and reference-type independence closely match the distinction between textual and archaeological evidence. That is, textual claims and archaeological claims are generally independent in the transmission-type and reference-type sense. This is important, since neither of these types of independence amounts to, or even indicates, epistemic independence. This last claim can be demonstrated systematically.

Transmission-type independence is not necessary for epistemic independence. This can be seen through an example of two authors, one with nothing to gain from a case for the credibility of the other, nonetheless commenting on the methods and abilities of the other. Dionysius of Halicarnasus, for example, explicitly describes the historical methods of Thucydides, thereby

giving us an epistemically independent account of Thucydides as a source of evidence. The type of informational link to the past is the same, as both Dionysius and Thucydides inform through written histories, but they provide importantly independent evidence nonetheless.

Transmission-type independence, as is common between textual and archaeological evidence, is not sufficient for epistemic independence either. This can be seen in the following sort of case suggested by Snodgrass.[15] Potsherds are often dated by reference to textual information, if, for example, there is a datable, textual reference to an episode or site which is relevant to the circumstances of finding the sherds. It would be circular then, and of no epistemic benefit from the use of independent evidence, to use the sherds to date the site or episode. In this case there are two types of information to cite as reporting on the date of the site, dating by textual evidence and dating by material evidence. It is easy to see, easy because we have sustained an epistemological perspective on the example, that any claim to independence in the evidence would be illusory. It is an intersection of textual and archaeological evidence, but the crucial information on dates has been put into the material evidence from the textual source. There is in this sense only one source of evidence for dating.

The conceptual point in this case is important. Information which arrives at the end of distinct types of channels is not necessarily independent in the relevant epistemic sense., The information and credibility of one source of information may have come directly, at some point before the final reception of evidence, from the other. I sometimes suspect this sort of thing is going on at the radio station I listen to, that the news reporter simply reads the local newspaper over the air. If this is the case then my reading the newspaper and listening to the radio supply different kinds of informational links to events in the world, but they are not epistemically independent. They involve exactly the sort of collusion that independent evidence is supposed to avoid. Radio and newspaper may in general be transmission-type independent sources of information, but further checking is needed to show that any two reports are epistemically independent. The same is true of texts and archaeology.

Similar examples and reasoning show that reference-type,

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[15] A. Snodgrass (1987) *An Archaeology of Greece* (Berkeley: 1987) 40.

like transmission-type, independence is neither necessary nor sufficient for epistemic independence. A brief look at each condition will be enough to show how the arguments can be made.

It is clearly possible to have two evidential claims about the same type of thing, or even about the same individual thing, that have separate justification. Separate authors, for example, could describe Athenian cleruchies or even mention the same particular cleruchy. Plutarch, Pausanius, and Diodorus all mention the cleruchy on Naxos, but as long as we have distinct reasons for accepting the credibility of each author, these are epistemically independent sets of evidence.[16] Neither reference-type, nor reference-token, independence is necessary for epistemic independence.

It is just as clear that claims about different things (different types or different tokens) are not always independently justified. That is, neither reference-type nor reference-token independence is sufficient to indicate epistemic independence. It is particularly likely that epistemic independence will be missing if a variety of evidence relies on a shared and limited collection of comparanda as the basis of evidential accountability. The ceramic evidence from a putative Athenian cleruchy may be used as a source of information on dating and perhaps function of the habitation sites. This is clearly distinct in reference from ceramic evidence at, say, a Classical tower in Attica. The two examples of evidence are likely though to share a reliance on comparison to pottery from dateable and understandable contexts such as the Athenian Agora or Kerameikos. This shared source of informational content and credibility of the evidence diminishes its epistemic independence.

Transmission and reference independence may yet turn out to be good places to look for epistemic independence if cases of the former are often contingently cases of the latter. The important point though is that the epistemically advantageous kind of independence does not follow automatically from differences of reference or informational link as are characteristic of the differences between texts and archaeology.

There is one other kind of independence with a tight connection to epistemic independence. Transmission-token independence is a necessary, though not sufficient, condition for epistemic independence. A shared line of information would invoke a shared step in justification between two evidential

[16] R. Meiggs (1972) *The Athenian Empire* (Oxford: 1972) 121–22.

claims. Thus to keep their justification separate, two evidential claims must come through distinct informational paths. As an example of this, consider two ancient authors. If it is discovered that one has used the other as a source of information, as is suspected of parts of Diodorus Siculus using Thucydides as evidence, then there is this shared channel of information and with it a shared aspect of justification. Insofar as transmission-token independence is compromised, so too is epistemic independence. But the distinction of transmission-token independence alone does not entail the epistemic separation, as a previous example proves. Since transmission-type independence is a subset of transmission-token, and since transmission-type is not sufficient for epistemic independence, it follows without any further need for examples that transmission-token is not sufficient for epistemic independence.

In sum, none of the other kinds of independence, physical or reference, amounts to the epistemic independence which is the kind of difference between cooperating claims which enhances the credibility of both. It is the explicitly epistemic form of independence which is at work in both ways of applying independent evidence referred to earlier. One claim may be descriptive of some aspect of the formation process of some other evidence. This sort of support of one evidential claim by another is a significant accomplishment only if the supporting claim does not covertly reap its epistemic benefit from the supported claim. This is what epistemic independence prevents. The other style of using independent evidence involves two claims about the same, or much more likely, different but related things. The two reports effectively collapse into one if it turns out that the justification for one is tied to the other. If the epistemic independence is lacking, two pieces of evidence are no better than one.

The result of all the details on types of independence and subsequently on the relationships among the types is that epistemic independence largely ignores the distinction between text and artifact, or between history and archaeology. Traditional disciplinary distinctions cannot be trusted as a demarcation of independent sources of information. Even the careful analysis of the substantive differences between texts and archaeology shows that these are not the right differences, the epistemic differences, which provide the advantage to using independent evidence. The distinctions between textual and archaeological evidence roughly match what I have called trans-

mission-type and reference-type independence. The two sources of evidence tend to report on different types of things, using information conveyed by different types of mechanisms. But neither difference secures epistemic independence. The only remaining possibility for an epistemic significance to the text/archaeology distinction is that transmission-type and reference-type independence are jointly sufficient for the epistemic task. But they are not, as a very general example can show. It is a staple of Greek archaeology to date sites largely from the evidence of potsherds. Thus the ceramic finds at, say, an Attic farmsite are evidence of the site's date. Compare this evidence to inscriptional, that is, textual, information which firmly establishes the dating for various levels and locations in the Athenian Agora. These two sets of evidence differ in both their reference and their mechanism of formation, yet they are very likely not epistemically independent. It is very likely that the ceramic material at the Attic farmsite bears information on dates because of its resemblance to comparanda found at the Agora and dated with reference to the texts. The credibility of the farmsite evidence depends in part on the textual evidence in the sense that refuting the latter would undermine the former. Thus reference-type and transmission-type independence are not even jointly sufficient for epistemic independence.

Behind all this logical analysis is the basic conceptual point that epistemic independence between evidential claims does not match the distinction between textual and archaeological evidence. There is independent evidence within archaeology. Evidence of dating by means of dendrochronology can be independently checked by means of analyzing varves. The function of a ceramic vessel can be understood by noting its shape and, as independent corroboration, by analyzing the context in which it is found. These sorts of examples, as well as cases of using independent claims as middle range theories to describe the formation process of evidence, are easy to find. So are cases of independence among strictly textual evidential claims. Several have already been listed above, as the case of two authors, neither dependent for material or credibility on the other, providing corroborating evidence, and the case of the Athenian Tribute List giving indirect evidence of Athenian cleruchies to be compared with more direct evidence of Plutarch, Diodorus, Pausanius, and others.

Pairs of textual claims can be independent, as can pairs of

archaeological claims, as can a mixed pair from texts and archaeology. Determining epistemic independence then is a case-by-case task. It requires an analysis of the formation process of the evidence and an account of the flow of information from the object in the past to the evidential record. The key to epistemic independence is the relation between these accounting claims and the final evidential claim itself.

CONCLUSION

The moral of the story is to not trust the separation of texts and archaeology with the determination of epistemic independence. It is only fair to end with the suggestion to not ignore it either. Using texts and archaeology as complimentary evidence does not insure independence, but it is a good place to look. Distinct and dissimilar formation process are more likely to lead to separately justified evidence. This makes it likely, though not guaranteed, that texts and archaeology can benefit each other as independent evidence. This can be done using either of the two styles of applying independent evidence, as separate pieces of the picture of the past, or as middle range theories, contributing to the account of the formation process of one another.

This second scheme, texts and archaeology as middle range theory for each other, is invoked fairly often, more frequently than is suggested by Leone and Potter in the comment, 'What is missing in even the best work so far is a method that solves the problem of how to array the documentary and archaeology data against each other productively'.[17] They advocate the middle range methods to take advantage of the independence between documentary and archaeological data. It is a good idea, and it is frequently carried out, though the middle range theorizing is rarely made explicit. A few brief, related examples will show both how independent middle range theorizing is done with texts and archaeology, and that it is a reasonably common place method.

A complex of farmsites near Karystos on southern Euboea is potentially material evidence of an Athenian cleruchy.[18] Much of the evidential relevance of the sites depends on the timing of their habitation, information which is provided largely by

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[17] Leone and Potter (1988) 9.
[18] Keller and Wallace (1986).

the ceramic remains both above and below the ground. The pottery gets its dating information by comparison with comparanda found in context with textually dated material such as inscriptions or buildings of textual reference. These texts are generally independent of the hypothesis of a cleruchy at Karystos, the hypothesis that the material evidence of Classical farmsites is intended to test. In such a case, the texts provide an informational link that gives meaning to the material evidence, and these texts, though never mentioned in reports on evidence for the cleruchy hypothesis, are playing the role of independent middle range theory for the archaeological evidence.

Extending the same example, material evidence of a cleruchy, demonstration of an organized and perhaps fortified complex of farms established around 450 BC and suddenly abandoned around 400, would be supportive of textual evidence for such Athenian outposts. Diodorus Siculus refers to cleruchs sent to Andros, Naxos, and Euboea (xii,22,2), and corroborating evidence through independent means, namely through the material evidence which is a credible reference to a cleruchy at Karystos, enhances the reliability of the Diodorus text.

A more subtle use of the Diodorus text and archaeological evidence as complimentary information is more in the form of middle range theory. Patterns in the material remains on Euboea show an association between the northern two-thirds of the island with the Greek mainland, and between the southern third of the island with the Cycladic islands. Southern Euboea participates in the activities of ancient Greece largely in association with the Cyclades. Thus when Diodorus refers to the dispatch of 1000 cleruchs divided between Andros, Naxos, and Euboea, there is reason to interpret this as a reference to southern Euboea. In this way, the material evidence enhances the meaning of the textual evidence, and it is based on archaeological claims about types of pottery which in no way rely on the writings of Diodorus for either meaning or justification. It is independently justified archaeological evidence for patterns of association used in the role of middle range theory for documentary evidence.

It is likely that this sort of analysis of the justification of evidential claims and the flow of information from the past to the present textual and archaeological records will show that the intersection of texts and archaeology as complementary middle range theories is quite frequent, though usually unre-

ported. It could, of course, be made even more frequent, to the benefit of both history and archaeology. The benefit is epistemic, both an enrichment of the meaning of evidence and a boost to its credibility, and it comes with the epistemic responsibility of checking each case for the particularly epistemic form of independence. Differences of discipline or of forms of evidence cannot be relied on for the epistemic authority expected of independent evidence. They are however indications of likely places to check.

HUSBANDRY, DIETARY TABOOS AND THE BONES OF THE ANCIENT NEAR EAST: ZOOARCHAEOLOGY IN THE POST-PROCESSUAL WORLD

BRIAN HESSE

INTRODUCTION[1]

The study of animal bones from archaeological sites in the Near East began in an intellectual matrix which emphasized historical, both documentary and aesthetic, and zoological approaches to their interpretation. These avenues produced important synthetic results, but bones remained peripheral to the central interests of the archaeologists of the day.

Then, beginning in the second quarter of this century, a different set of archaeological perspectives took hold in some sectors of Near Eastern archaeology. These were materialist and self-consciously scientific (later often called 'processual') in outlook. They emphasized, in varied and nuanced ways, that social and political institutions and processes were embedded in ecological and economic relations. Processual accounts of the past were depersonalized. Perturbations in the working of cultural systems replaced an understanding of choices made by human actors in an historical context as the mode used to account for innovation. Further, processual accounts focussed on the comparative efficiency of cultural systems in explaining change. The relations between humans and their environment and the productivity of different adaptive systems in generating energy were treated as especially significant, while the relations between individuals and the social and ideological value of things were considered less, if at all. While originally focussed on such questions of prehistory as the 'neolithic revolution,' this non-historical approach gradually spread to the archaeology of the earliest literate 'civilizations.'

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[1] This paper had its origin in one entitled 'Post-Processual Zooarchaeology: Zoo-Ideology and Zoo-Politics in Ancient Canaan' given by the author and Paula Wapnish at the Annual Meeting of the Society for American Archaeology in April, 1989.

The new way of thinking put a premium on the collection and interpretation of biological remains from archaeological sites. The number of zooarchaeologists, many from anthropological backgrounds, working in the region sharply increased and a body of descriptive and interpretive accounts in this mold has been produced.

Now, however, in the late 1980's and 90's, the ecological paradigm in turn has come under sharp attack and a shift to what are called 'post-processual' themes that emphasize, among other things, relativistic, volitional, gender sensitive, historical, Marxist or symbolic accounts, has been advocated. A question is whether zooarchaeology can adapt to these changes and contribute to the discourse about the past without returning to its previous position on the fringe. Following a brief survey of animal related studies in ancient Near Eastern history and archaeology, two problems are explored, one a traditional 'processual' question, the advent of animal domestication, the other a traditional 'historical' question, the biblical tradition of pig avoidance, to suggest that both the new archaeology strategy still has utility and that a post-processual Near Eastern zooarchaeology is possible.

Background

Examined at a global scale, zooarchaeology's intellectual roots have drawn more sustenance from external demands for a method than they have from any theories about the past that have been generated within the discipline. As can be argued as well for the companion specialists, paleobotanists, zooarchaeologists find self-definition within the society of archaeologists through the class of material remains we are competent to study. Bone fragments are often the second most common find in Near Eastern excavations, and it can be argued that the right to discover carries the companion responsibility to study. On a crude level, therefore, since bones are found, zooarchaeologists exist.

As biological specialists, zooarchaeologists have sprung into existence to exploit the niche that has formed between the temporal boundaries that separate paleontologists, whose interest fades as the Pleistocene comes to an end, from neontologists, whose primary focus is on living populations of animals, and the division of labor among biologists that determines what zoologists, focussed on wild species, and animal scientists, tar-

geted at modern veterinary and husbandry problems, are prepared to study. As a practical matter this zooarchaeological 'turf' contains and concerns subrecent, often domestic, animals known almost exclusively from hard tissue remains. In the past, the shifting theoretical currents within archaeology have made this niche more or less important to inhabit and shaped the adaptive strategies employed to exploit it.

Zooarchaeology has not been the sole discipline studying the fauna associated with the human past. There have been three approaches to the study of the animal world of the ancient Near East—history, zoology, and, what I will label here 'archaeological science'. While these intellectual strategies emerged sequentially, they overlap more than replace one another, and all continue today to have productive adherents.

Approach 1—History. For more than a century scholars have been concerned with the animal world that surrounded, affected and interested the people and cultures of the ancient Near East. Some workers, including art historians, historians, and philologists, instituted, as an approach to this subject, the study, sometimes integrated, of the aesthetic and documentary productions of the past societies of the region.[2] Their careful consideration of the context of terms and images has produced a detailed record of how individual animal types were conceptualized by the cultures of the ancient Near East.

This scholarship, insofar as it deals with the primary physical evidence of past animals, bones found in archaeological sites, at all, does so sparingly, employing osteological observations mainly as peripheral information supportive of conclusions drawn from the cultural record. Publications have frequently been organized on the basis of zoological taxonomy rather than culture history—sections devoted to horses, goats and pigs rather than ones descriptive of, say, the Old Kingdom, Neo-Babylonian

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[2] A small sampling of this work spread over the past 120 years would include W. Houghton (1877) 'On the Mammalia of the Assyrian Sculptures', *Transactions of the Society of Biblical Archaeology* V (1877) 33–64, 319–383; J.U. Dürst (1899) *Die Rinder von Babylonien, Assyrien und Ägypten und ihr Zusammenhang mit den Rindern der alten Welt,* (Berlin: 1899); B. Landsberger (1934) *Die Fauna des Alten Mesopotamien nach der 14. Tafel der Serie ḪAR-RA-Ḫubullu, Abhandlungen der Philologisch-Historischen Klasse der Sächsischen Akademie der Wissenschaften* 52, No. 6 (1934); E.D. van Buren (1939) *The Fauna of Ancient Mesopotamia as Represented in Art, Analecta Orientalia* 18 (1939); A. Salonen (1976) *Jagd und Jagdtiere im Alten Mesopotamien. Annales Academiae Scientarium Fennicae.* Series B, 196 (1976); and E. Porada (1990) 'Animal Subjects of the Ancient Near Eastern Artist', in *Investigating Artistic Environments in the Ancient Near East,* ed. A.C. Gunter (Washington: 1990) 73–79.

Period and Iron Age. Additionally, the discussion often has been atomized into species by species vignettes. Little interest has been placed on discerning the relative importance or contribution of each of the taxa in some historical or regional setting, or how the various taxa were assembled into what might be termed *cultural faunas*, the suites of animals which together supported and were conceptualized by ancient societies. In fact, due to their salience in imagery and literature, wild animals and those species that were rare or exotic in the environment of the ancient Near East have received a disproportionate share of scholarly treatment.

In a few cases, facts about animals have been central to historical studies. For instance, the development of a caravan traffic linking southern Arabia and the Mediterranean was a significant economic and political development in the early first millennium BC. The distribution of camel bone finds and camel related artifacts and texts has been used as primary evidence in dating this event.[3] Also, as I will discuss in some detail below, pig bones have been used by some scholars as both ethnoideological markers and evidence for the origins of specific religious law in the history of Palestine. However, outside these and a few additional examples, the broader historical and processual trends and patterns in the strategies that structured past human interest in and use of animals are difficult to discern in the historical literature devoted to animals.

These remarks are not meant as criticism. Many of the publications that have resulted from the historical approach to the animals of the ancient Near East are paradigms of scholarship. Moreover, workers in this arena have been willing (as have the zoologists mentioned below) to tackle the questions of animals in the Near East on the broadest scale, something anthropologically oriented archaeologists have not yet been able to accomplish. It is rather my point that because these studies lack a sociological or anthropological grounding, they emphasize a view of culture that is primarily linguistic and aesthetic, if not elitist. As such, they do not as effectively express the economic and political significance of animals to past cul-

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[3] See the discussion in Ch. 3 of R. Bulliet (1975) *The Camel and the Wheel* (Cambridge MA: 1975). For a more critical treatment of the problem from a zooarchaeologist—historian, see P. Wapnish (1981) 'Camel Caravans and Camel Pastoralists at Tell Jemmeh', *Journal of the Ancient Near Eastern Society of Columbia University* 13 (1981) 101–21.

tures as they do the ideological and symbolic content of zoological images and terms.[4] Additionally, these early studies are founded in an intellectual style where the explanations for changes in ancient Near Eastern and Egyptian civilizations minimized the significance of internal change in cultural development, emphasized the activities and decisions of individual leaders in redirecting the course of society, and were largely rooted in diffusionist models that traced innovations to external sources.[5]

Approach 2—Zoology. Other scholars, a group that includes many who came to the subject from a zoological background, focussed on the study of the fragmentary animal bones excavated from archaeological sites. However, they have done so in a way that emphasizes the animal side of the topic—a sort of veterinary zooarchaeology.[6] The chronology and geography of the abundance, distribution, and morphology of various animals have been carefully explored on a site by site basis with some attention to the impact humans have had on these variables, but the opposite side of the coin, the distribution of these variable animal qualities in terms of the social, economic,

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[4] More recently, however, text scholars, in particular, have examined documents for the significance they hold for reconstructing the productive systems that underlay ancient societies. The volumes of the *Bulletin on Sumerian Agriculture,* while containing many contributions that continue the tradition of organizing information by biological categories, also includes studies that are focussed on agricultural production systems. In the realm of animal studies, the work of M.A. Morrison (1981) 'Evidence for Herdsmen and Animal Husbandry in the Nuzi Documents', in *Studies on the Civilization and Culture of Nuzi and the Hurrians,* eds. M.A. Morrison and D.I. Owen (Winona Lake IN: 1981) 257–296; and F.R. Kraus (1966) *Staatliche Viehhaltung im Altbabylonischen Lande Larsa, Mededelingen der Koninklijke Nederlandse Akademie von Weteschappen* 29 (1966) can be mentioned.

[5] This is not the place for an historical review of theory in old world archaeology. B. Trigger's book (1989), *A History of Archaeological Thought* (Cambridge: 1989) provides an eloquent and insightful review of the issues. Suffice it to say that the historical approach associated with the Annales School with its emphases on patterns of historical change greater than the event has not been evident in the archaeology of the Ancient Near East until recently, see the discussions in A.B. Knapp's edited work (1992) *Archaeology, Annales, and Ethnohistory* (Cambridge: 1992).

[6] This literature is also extensive. Representative publications would include for the ancient Near East and Egypt such works as F.S. Bodenheimer (1960) *Animal and Man in Bible Lands, Collection de Travaux de l'Académie Internationale d'Histoire des Sciences* 10 (Leiden: 1960); J. Boessneck (1988) *Die Tierwelt des Alten Ägypten* (Munich: 1988); and B. Brentjes (1965) *Die Haustierwerdung im Orient, Neue Brehm Bücherei* 344 (Wittenberg Lutherstadt: 1965); as well as the global summaries by F.E. Zeuner (1963) *A History of Domesticated Animals* (London: 1963); and J. Clutton-Brock (1981) *Domesticated Animals from Early Times* (Austin: 1981).

and political institutions of those who exploited them, has been less well developed. A useful result of this work has been to stimulate greater care in the choice of names applied to images in ancient art.[7] An additional exceptionally important general contribution has been the steady development of techniques for the identification and morphological evaluation of osteological finds. Another has been the description and publication of substantial bodies of zooarchaeological data. A glance at one of the general bibliographic sources for the study of ancient animal bones[8] will indicate that the bulk of the osteological facts about the past of the Near East that we have at our disposal were generated by this approach. However, it is also fair to say that much of this work paid less attention to the archaeological complexities of the problem, in particular the within site synchronic variability of animal remains and the biases of differential preservation and archaeological recovery, than many, particularly those who advocate the third approach that is described next, would have liked.

Approach 3—Archaeological Science. Ever since the 1930's an emergent theme in archaeological theory has been the environmental or ecological approach.[9] Rooted in functionalism, this movement took root and blossomed into the 'New Archaeology' in the 1960's, particularly amongst anthropologists. Emphasizing, in its fully developed form, on the one hand, the integrated reconstruction of social, political and economic systems and, on the other, the goal of discovering ever more general laws about human behavior, this approach disdained historical approaches to explanation as particularistic, dependent on the authority or reputation of the historian, and nonscientific.

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[7] An annoying (to me) case of improper labeling persists. The wild goat of Anatolia, western Iran and northern Iraq, *Capra aegagrus*, is properly referred to as the Bezoar or Pasang, not the Ibex. This latter form, *Capra nubianus*, is a denizen of the hill country of the eastern Mediterranean, Arabia, and the western border of the Red Sea. See figure 6 and 9 in R. Valdez (1985) *Lords of the Pinnacles, Wild Goats of the World* (Mesilla, NM: 1985). Thus to refer to Mesopotamian wild goat images as 'Ibex' has only the most remote chance of being accurate.

[8] As examples, H.R. Stampfli and J. Schibler (1991) *Bibliography of Archaeozoology* (Basel: 1991) and H.-H. Müller, *Bibliographie zur Archäo-Zoologie und Geschichte der Haustiere* (Berlin: Zentralinstitut für alte Geschichte und Archäologie, annual since 1958). The enormous contribution of the late J. Boessneck and his colleagues and students to our knowledge of the zooarchaeology of Egypt and the Near East deserves special mention.

[9] See Trigger (1989) Chapter 7.

The explanatory emphasis on the environment found some fertile ground in Near Eastern prehistory, particularly in the study of the 'Neolithic Revolution'. Robert Braidwood's interdisciplinary team approach to the excavations at Jarmo in Iraq was a watershed in fostering the integration of zoological expertise into the fieldwork phases of archaeology.[10] In the prehistories that were written following this approach, zooarchaeological contributions were fundamental, necessary records of material variables that were seen as critical to the functioning of ancient cultural systems.

In the archaeology of the historic periods in the Near East, the integration of the Archaeological Science approach has been far less complete. One positive development has been the development and application of taphonomic theory, a project accomplished in tandem with behavioral archaeology.[11] Zooarchaeology has helped draw attention to the complexity of site formation processes and the impact of excavation techniques on the character of recovered assemblages.

However, a cynic also might argue that in the eyes of some 'regular' archaeologists of the historic periods, bone specialists are more like *talismans.* They are largely *symbols,* visible indications of a research project's participation in scientific archaeology, even when, on closer examination, it is not clear how the excavators expect to incorporate the biological data into their general accounts of the results of excavation. One symptom of this condition is the appendix style publication of animal bone reports. Another is the occasional departure of a zooarchaeologist from a research project, a decision often stimulated by frustration in attempting to influence excavation strategy and collection protocols. Zooarchaeology as an archaeological science is intensely quantitative. Little good is generated by

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[10] An early example of the team approach to environmental archaeology that Braidwood developed is Robert J. Braidwood and B. Howe (1960) *Prehistoric Investigations in Iraqi Kurdistan,* SAOC 31 (Chicago: 1960). Included in that work are chapters by Charles Reed, 'A Review of the Archaeological Evidence on Animal Domestication in the Ancient Near East' (pp. 119–146), and Reed and Braidwood, 'Towards the Reconstruction of the Environmental Sequence of Northeastern Iraq' (pp. 163–174), that were important statements in the development of the 'Hilly Flanks' theory which tied experimentation with domestication to environmental richness.

[11] Here I am referring to the 'old' behavioral archaeology associated with M.B. Schiffer (1976) *Behavioral Archaeology* (New York: 1976) rather than the more recent use of the term by T.K. Earle and R.W. Preucel (1987) 'Processual Archaeology and the Radical Critique', *Current Anthropology* 28 (1987) 501–38.

applying its methods to samples compromised by poorly controlled excavation.

On the other hand, there have been successful collaborations where bone specialists' *magic*, the special knowledge of biological arcana required to exercise the new archaeology's theoretical agenda, was achieved.[12] A particularly well developed example is provided by Wright, Redding and Pollock's integration of animal remains, artifacts and stratigraphy to illustrate the strategy for responding to economic stress used by an early complex society in Iran.[13]

However, as in the case of Wright and his co-workers, even today, almost all major Near Eastern animal bone studies that touch the historic periods and which can be assigned to the Archaeological Science approach are focussed on the earliest phases in the development of complex society.[14] Despite the demonstrated potential, zooarchaeologists have remained peripheral in the study of later periods, more or less defined by contributions to method and limited in theoretical scope to the world of animals. Part of the problem is clearly the daunting complexity of the textual record that is related to the animal economies that had developed in Mesopotamia by the end of the third millennium. Zooarchaeologists need to learn, as have historical archaeologists in other regions, that they need not construct all of the categories of analysis from their own material, the written record can provide a structure (see the article by P. Wapnish, this volume).[15] However, another is the need to recognize that documents are a severely biased record of the exploitation of animals. Interpretation demands that patterns derived from written sources be evaluated against the

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[12] In B. Hesse and P. Wapnish (1985) *Animal Bone Archaeology, From Objectives to Analysis* (Washington: 1985) 1–2, figs. 1 and 2, we argue that successful zooarchaeology is organized around cultural questions rather than ecofactual categories.

[13] H.T. Wright, R.W. Redding, and S.M. Pollock (1989) 'Monitoring Interannual Variability: An Example From the Period of Early State Development in Southwestern Iran', in *Bad Year Economics, Cultural Responses to Risk and Uncertainty*, eds. P. Halstead and J. O'Shea (Cambridge: 1989) 106–13.

[14] Additional examples of the integration of zooarchaeological perspectives in the study of early complex societies are R.G. Killick, ed. (1988) *Tell Rubeidheh, An Uruk Village in the Jebel Hamrin, Hamrin Salvage Project Report* 7, *Iraq Archaeological Reports* 2 (Wiltshire, England: British School of Archaeology in Iraq, Directorate of Antiquities, 1988) and M.A. Zeder (1991) *Feeding Cities, Specialized Animal Economy in the Ancient Near East* (Washington: 1991).

[15] J. Deetz (1977) *In Small Things Forgotten* (Garden City NY: 1977) 13.

behavioral record. Negotiating these barriers will require more collaboration of the form pioneered by Postgate and Paine in their joint examination of herding contracts.[16]

One question to be addressed below is whether the points of view of archaeological science can continue to penetrate to later historical periods of the ancient Near East with similar effect. The example considered below is one in which the negative attitude toward an animal, the pig, reported in ancient textual accounts has often been explicated in ideological or theological terms. As will be shown shifting the discussion to the material bases so favored by the processual approach provides alternative explanatory insights related to economic, social and political institutions.

Approach 4—Post-processual Zooarchaeology? A major critical challenge, usually labelled *Post-Processual,* to the enabling theory of the third approach of archaeological science has arisen. This multi-bodied discussion includes a host of perspectives that range from deconstruction to feminism, from symbolism to the kaleidoscope of Marxist approaches, as well as the resurrection of the historical perspective. It takes as its target much of the theoretical infrastructure which has justified and funded the application of zooarchaeology to cultural rather than strictly zoological, textual, or artistic questions.[17] The post-processual critique demands that archaeology shed its dependence on ecological modeling, optimal foraging theory, and the other emphases on the human to resource interface, and adopt a stance that emphasizes meaning as it is constructed socially and expressed materially. Kevin MacDonald provides a succinct statement of the self-criticism the post-processual challenge has produced within zooarchaeology itself:

> Within all the approaches..., the need for access to the more ideological realm behind the structuring of bioarchaeological data has been presented as a tantalizing possibility. At least in England, this idea finds its root in the early attempts of post-processualists to 'hit Palaeoeconomy where it hurts' (in the economy of course...).[18]

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[16] J.N. Postgate and S. Payne (1975) 'Some Old Babylonian Shepherds and Their Flocks', *Journal of Semitic Studies* 20 (1975) 1–21.

[17] The range of perspectives encompassed by the Post-Processual critique defies brief summation. An excellent survey of the field is provided by I. Hodder (1991) *Reading the Past,* 2nd edition (Cambridge: 1991).

[18] K. MacDonald (1991) 'Archaeozoology as Anthropology?', *Archaeological Review from Cambridge* 10 (1991) 60–69.

Given the possibility that we are actually in a Kuhnian paradigm crisis, it seems reasonable to ask whether the totem of zooarchaeology is *Anolis carolinensis*, the Chameleon, a source of power to survive the shift to the new modes of archaeological thought, or whether the field is constructed in the image of *Raphus cuculattus*, the Dodo, and thus doomed to extinction (or at least lack of funding!).

Can the growth in zooarchaeological research be maintained in this new theoretical climate or is expansion of the discipline entirely dependent on the requirements of archaeological science? Perhaps the answer is no. One criticism of the general utility of post-processual approaches is that meaning based archaeology is achievable only in the presence of contemporary documents or, at least, in settings where analogy can be undertaken through the direct historical approach.[19] This position leads inevitably and, in my view, unfortunately, to an even firmer establishment of the intellectual divide that already separates prehistoric from historic archaeology. I would prefer to agree with Young that the distinction between preshistoric and historical archaeology is a false one, that:

> There is not much difference between the archaeological and documentary sources.... They pose similar problems of interpretation, and neither, as a result, has any greater validity in our efforts to reenact the past than the other.[20]

Yet I admit that the fact remains that existing methods of exposing intentionality at the level of the individual or the social class from non-texual remains are severely limited. Others would say that we have not yet solved adequately the problems of site formation processes. The probability that we can extract social and ideological links to our osteological variables remains low. This argument turns post-processual zooarchaeology into a project for the future, allowing interest to resettle on the zoological questions associated with the second approach to research on ancient animals discussed above.

On the other hand, perhaps the answer is yes. However, as widely shared characteristics of the post-processual school are commitment to relativism and denial of universal epistemologies,

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[19] This was a theme developed by B. Styles in her discussion of papers delivered at the 'From Bones to Behavior' symposium at Southern Illinois University, Carbondale in April 1991.

[20] T.C. Young, Jr. (1988) 'Since Herodotus, Has History Been a Valid Concept?', *American Antiquity* 53 (1988) 11.

problem choice, or interpretive models, it seems profitless to attempt some sort of theoretical accommodation of all the themes of the radical critique, that is to generate a *radical zooarchaeology*. What would appear best is to do what zooarchaeologists have always done, follow Johnson's exhortation to 'get on with [the] job, whatever we might variously define the job to be'[21] and press the perspectives of the new theories against that which has been used to construct our intellectual world, bones. Out of that we should be able to see if there is profit in loosening the ties to the traditional new archaeology.

Several post-processual themes are considered here in relationship to zooarchaeological issues in a specifically prehistoric context. The first is the need to re-introduce volition into our accounts of the causation behind technological choices as they affected the human exploitation of animals. The factors that we claim induced societies to make fundamental alterations in their ways of life must be shown to have been perceivable by the actors who made the choices. Second, processual accounts of animal management or exploitation strategies have tended to overlook the internal social dynamics of choice—how some strategy benefits one sector of society at the expense of another, a more Marxist version of materialism—emphasizing rather the overall energetic gain of an innovation to the community at large. Third, if we are to succeed *in explaining the record obtained from a particular site*, we must shift from the search for ultimate causes to the consideration of proximate forces, those that operate in an historical context. The statistical unlikelihood that we ever actually excavate an instance of an initial and pristine innovation means that we must consider the possibility that what we take as invention is actually diffusion and calling for a different explanatory approach.

DOMESTICATION

Explaining the incorporation, whether origin or adoption, of domestic stock into the subsistence strategies of ancient groups has always been a special research question of animal bone archaeology.[22] In what follows, I will attempt to expose some

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[21] G. Johnson (1987) Comment on T.K. Earle and R.W. Preucel, 'Processual Archaeology and the Radical Critique', *Current Anthropology* 28 (1987) 517–18.

[22] The review of S. Angress and C.A. Reed (1962) *An Annotated Bibliogra-*

inadequacies of the treatment of this problem advocated by many adherents of the archaeological science approach. Further, I will suggest that attention to the perceptions of the ancient actors, a shift in focus away from animals as food to their utility in negotiating social strategies, and an emphasis on the informational complexities of pastoralism, all elements of the post-processual critique and accessible only through an historical perspective, provides a fuller model of options to account for the advent of the husbanding lifestyle at sites in the Early Holocene Near East.[23] My goal here is only to contribute toward the reformulation of our understanding of the domestication process. As will be plain, much reworking of the excavated record will be required to implement these suggestions.

Nearly universally, models of the domestication process in the Near East have focussed on the production of caloric resources that such an alternative provides as the incentive pushing people to innovate.[24] Since the 60's the proposed processual explanations for the domestication of herd animals have emphasized the fact that pastoral production (despite the expense of higher labor costs) could provide relief from stress either through higher potential production levels, a tack preferred by those who are impressed by the likelihood for destructive population growth among sedentarized societies,[25] or the redirection of risk, a line of discussion followed by those who find population explosions hard to document but complex and hazardous environments easy to find.[26] Whatever the model, the primary link is between consumer and consumed, not the relationships between consumers.

As an example of these approaches consider the late Ken Russell's effort to apply an optimization model constructed from

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phy on the Origin and Descent of Domestic Mammals 1900–1955, Fieldiana: Anthropology 54 (1962) demonstrates the antiquity of the literature.

[23] The potential of social and ideological models to the study of domestication has been more quickly adopted in European settings than in the Near East. The reason for this is not entirely clear. However, the relative decrease in fieldwork at Neolithic sites in large parts of the region brought on by political instability may be a factor.

[24] See the review by P.J. Crabtree (1993) 'Early Animal Domestication in the Middle East and Europe', *Archaeological Method and Theory* 5, ed. M. Schiffer (1993) 201–45.

[25] Examples of this approach include M.N. Cohen (1977) *The Food Crisis in Prehistory* (New Haven: 1977) and P.E.L. Smith (1976) *Food Production and Its Consequences*, 2nd edition (Menlo Park CA: 1976).

[26] See, on the question of population pressure, M.A. Glassow (1978) 'The Concept of Carrying Capacity in the Study of Culture Process', *Archaeologi-*

ethnographically based estimates of the subsistence efficiencies of various pastoral strategies to explain the sequence of appearance of domestic species in the Middle East.[27] According to his scenario, additional, and less efficient, herd animals are added to the pastoral mix as the environmental or other limits to the exploitation potential of previous domesticates are reached. Russell makes a valuable contribution by demonstrating the probable early significance of dairy production in neolithic subsistence strategies. He, however, focusses on the energy gains achieved through milk/cheese production. I will suggest below that what may be more important is the social implications of such an innovation.

Unfortunately, Russell's overall predicted sequence for the addition of species to the list of husbanded forms does not coincide with the understanding of the chronology of pastoral evolution that has been worked out over the past decades by a host of investigators. To save his thesis that marginal efficiencies are crucial in explaining changes in subsistence strategies, he is forced to an elaborate argument about how the expectations of previous workers have created a misrepresentation of the histories of cattle and sheep/goat domestication. Curiously, given its presence in a strongly evolutionary and optimal foraging theoretical setting, this is a kind of 'radical critique' itself, insisting as it does, that the predispositions of previous investigators have led them to fail to objectively interpret the record before them. One wishes to ask: couldn't the failure to predict the sequence of pastoral development invalidate this formulation of optimization theory as easily as it questions the reliability of other scholars?

From the perspective of the 'post-processual' schools, much is wrong with these approaches. For example, by focussing on what O'Brien and Wilson admiringly refer to as 'ultimate' rather than 'proximate' causes,[28] human intentionality, the historical

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cal Method and Theory 1, ed. M. Schiffer (1978) 32–48. On variable environments see B.C. Hesse (1978) 'Evidence For Husbandry From the Early Neolithic Site of Ganj Dareh in Western Iran', (Ph.D. Dissertation, Columbia University, 1978); and, R.W. Redding (1988) 'A General Explanation of Subsistence Change: From Hunting and Gathering to Food Production', *Journal of Anthropological Archaeology* 7 (1988) 56–97.

[27] K.W. Russell (1988) *After Eden. The Behavioral Ecology of Early Food Production in the Near East and North Africa*, BAR International Series 391 (Oxford: 1988).

[28] M.J. O'Brien and H.C. Wilson (1988) 'A Paradigmatic Shift in the Search for the Origin of Agriculture', *American Anthropologist* 90 (1988) 858–965.

context in which decisions are taken, is specifically and intentionally bypassed. In the strongest form of this argument, it has been advanced that domestication is a subtle Darwinian process. Incremental biological changes, in both human populations and the plant and animal communities within which they live, are brought on by their interaction and resultant interwoven selection. In time, domesticated plants, animals and humans emerge from this co-evolutionary process.[29] These explanations elegantly account for the biological features of domestication but do not address the cultural concept of domestication. Thus, alternatively, it is argued, in essence, that since domestication produces higher yields, a form of insurance, or at least a redirection of risk, those groups which adopt the technology will ultimately outstrip their foraging competitors. Domestication will be selected for economically and spread from society to society. Not only does this model assume that these advantages to domestication are true from the onset of the innovation, the argument also defines all the concepts in the analysis—resource, risk, stress, strategy—outside the past social situation in which they are supposed to have had force. That is, they are only certainly real in the minds of the investigators. As a result, social institutions and social values, the tradition bound 'ways of doing things' are not seen as impediments to the adoption of the new technology. Material advantage inevitably leads to a technology's adoption. Explanations advanced in this value vacuum for the ancient actors, with no historical texture of choice and decision, are reductionist and ultimately unsatisfying. Steven Mithen puts it well,

> cultural behaviour derives from capacities for learning, decision making and problem solving. As biological endowments these reside in the individual. Consequently, explanations for cultural behaviour require explicit reference to decision making by individuals.[30]

As Bennet Bronson pointed out almost two decades ago, risk and stress have to be *perceived* to influence choice. He writes:

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[29] For agriculture, see D. Rindos (1984) *The Origins of Agriculture, An Evolutionary Perspective* (New York: 1984); for animal domestication, E. Tchernov and L.K. Horwitz (1991) 'Body Size Diminution under Domestication: Unconscious Selection in Primeval Domesticates', *Journal of Anthropological Archaeology* 10 (1991) 54–75.

[30] S.J. Mithen (1990) *Thoughtful Foragers, A Study of Prehistoric Decision Making* (Cambridge: 1990) 256.

> As modern specialists on agricultural development have begun to emphasize, what counts most in subsistence decisions is PERCEPTION. If a technique is perceived to be laborious then it will be resisted even if, from the standpoint of an outside observer, it is convenient and economical.[31]

Perceptions of the potential adoptability of an animal management technique, depend, at least in part, on the complexity of the task and the difficulty and risk of learning it. Even more broadly, by reducing archaeology to evolutionary biology, by focussing in on FOOD production, the non-caloric exchange values of a resource are systematically bypassed. In this way potential tensions in the social or ideological fabric of a society that could generate or resist new animal related activities are invisible, even though they are materially constituted. This is because they are not necessarily linked to variable efficiencies in resource production seen at the consumer/producer level.[32]

Two ethnographic examples make this consideration more concrete. Sperling has described that the diversification into camel production by East African cattle pastoralists, the Samburu in the following way:

> There is nothing easy or natural about cattle pastoralists adopting camels. Stockowners who start to build up herds recount three central problems: they have difficulty obtaining camels, labour problems in pasturing them, and no experience in either preventing disease or treating sick animals.... By whatever calculation one uses, camels are expensive. For those who know little about them, the move to purchase a 'two—cow' or 'seventeen—goat' experiment is a risky one.[33]

These problems restrict the diversification into camels to those economic sectors of Samburu society which are best able to afford and bear the risk of the investment. Even if the shift is seen as a response to the ever more damaging region wide drought, it is the least affected sector of the community that makes the subsistence change.

It seems likely that a critical factor in the adoption of this complex technology is the pre-existence of social stratification

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[31] B. Bronson (1975) 'The Earliest Farming: Demography as Cause and Consequence', in *Population, Ecology and Social Evolution*, ed. S. Polgar (The Hague: 1975) 61.

[32] See, in this regard, B. Bronson (1972) 'Farm Labor, and the Evolution of Food Production', in *Population Growth*, ed. B. Spooner (Cambridge MA: 1972) 190–218.

[33] L. Sperling (1987) 'The Adoption of Camels by Samburu Cattle Herders', *Nomadic Peoples* 23 (1987) 10–12.

in the distribution of wealth. Domestication is a strategy open to 'investors', those who can afford to fail. An often forgotten point about animal domestication is that it does not produce immediate results.[34] The ethnographic record is replete with instances that indicate that successful herders are those who can accumulate large herds. Resisting the urge to slaughter stock is crucial. This value contrasts with the sharing ethic usually associated with foragers as they distribute meat from a successful hunt. Thus it is hard to see how taking up husbandry can be a rational choice for hunter/gatherers faced with a resource shortfall—not only would they be expected to share what animals they could capture, but the demands of a pastoral economy require that they not eat them at all.

However, taming animals does make sense in a context of an uncertain food supply, providing as it does both storage and diversification, two of the main buffering mechanisms used to respond to variability.[35] It is here where Ken Russell's argument that dairy production was significant in the earliest phases of domestication in the Near East is important. Husbanding bovids for milk would have been an ideologically acceptable mechanism of incorporating stock within the social confines of a hunting/foraging society. As Tim Ingold has pointed out, stock managed for such a purpose could be used a basis for the subsequent development of meat focussed production.[36]

If such a model of investment in dairying as a mechanism for the development of domestic stock is applicable, evidence should be found in the more complex early Neolithic sites where some degree of social stratification can be by hypothesized. However, an animal bone record for such a process will be difficult to discern since, often, the species that was most significant to the hunting economy is also the animal that was domesticated. Thus, if small numbers of domestic sheep were maintained by a society of sheep hunters, the remains of the few tame animals will be masked by the abundance of

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[34] See my discussion in 'Carnivorous Pastoralism: Part of the Origins of Domestication or a Secondary Product Revolution?' in *Culture and Environment: A Fragile Coexistence*, eds. R.W. Jamieson, S. Abonyi, and N.A. Mirau, pp. 99–109 (Calgary: 1993).

[35] P. Halstead and J. O'Shea (1989) 'Introduction: Cultural Responses to Risk and Uncertainty,' in *Bad Year Economics*, eds. P. Halstead and J. O'Shea (Cambridge: 1989) 3–4. See also, R. Huss-Ashmore, J.J. Curry and R.K. Hitchcock, eds. (1988) *Coping with Seasonal Constraints* MASCA 5 (1988), and I. de Garine and G.A. Harrison, eds. (1988) *Coping with Uncertainty in Food Supply* (Oxford: 1988).

[36] T. Ingold (1974) 'On Reindeer and Men', *Man*, n.s. 9 (1974) 523–38.

those taken as game. At Abu Hureyra, a Neolithic site on the Euphrates in northern Syria however, evidence may exist. Here, small numbers of sheep bones are found in deposits that contain mostly gazelle remains.[37] The bone collections from later phases of the occupation at the site contain large numbers of domestic sheep. While we are not in possession of enough information about the early Abu Hureyra sheep to be certain either about their domestic status or, if they were tame, to what purpose that were put, the evidence is tantalizing.

Another issue related to domestication is learning. Rogers observes that there is a reduction in the power of acultural models to explain complex technologies that are dependent on social learning (tradition) to transmit compared to those techniques which can be acquired individually.[38] This point can be illustrated in both the pristine and secondary context. As an example of the first, Yemenite villagers have on occasional attempted to husband hyraces (*Procavia capensis*), a large rodent sized relative of the elephant!, dwelling in colonies in rocky, rough terrain in Arabia and the eastern Mediterranean, as an alternative food and source of cash. These experiments appear to have failed as much from inadequate knowledge of hyrax management as from the low potential productivity of the species.[39]

An example of the secondary context returns us to the case of the Samburu, where access to expert advice from the neighboring Rendille is important in innovation:

> From the time of his birth, a Samburu is taught to think about, act with, and love cattle, sheep and goats. He has years to learn about his charges; he can tap into a legacy of herding lore and

[37] A.J. Legge and P. Rowley-Conwy (1986) 'New Radiocarbon Dates for Early Sheep at Tel Abu Hureyra, Syria', *Oxford Committee for Archaeology Monograph* 11 (1986) 22-35.

[38] A. Rogers (1988) 'Does Biology Constrain Culture?', *American Anthropologist* 90 (1988) 819-31.

[39] This example has been discussed more fully in T.B. Stevenson and B. Hesse (1990) '"Domestication" of Hyrax (*Procavia capensis*), in Yemen', *Journal of Ethnobiology* 10 (1990) 23-32. It is worth noting that this indigenous experimentation with novel domestication took place in a social context of significant economic stratification as a strategy of the disadvantaged to exploit the market provided by the more well off. International development agencies also have encouraged the adoption of a new domesticates, in particular those of small body size and high reproductive rates as a mechanism by which the poor can gain access to the market, (see National Research Council (1991) *Microlivestock: Little-Known Small Animals with a Promising Economic Future* (Washington: 1991) 5-14).

> technical expertise; and he is often guided by those more knowledgeable than he. The new camel owner, in contrast, is relatively alone. . . . New camel owners may decide to farm out their one or two animals. . . . These experienced camel herders provide the additional services of a bull and may oversee the first birth of their new charges. . . . More generally, a bond is established between the veteran and novice herder which allows the former, in particular, to ask for aid in the future. . . . The camel expert continues to play the role of advisor and, for many years to come, he may receive the camels at crucial times of sickness or birthing.[40]

In other words, from two perspectives—the potential for experimentation and the social process of learning a complex task, the relations among people are at least as essential as the relations between people and their resources in the development of animal management.

Another recently published ethnographic example provides a picture of pastoral adoption by a formerly cave dwelling, foraging society. Lee Cronk reports that the Mukogodo of north-central Kenya adopted cattle pastoralism in the first part of this century. For my purposes what is especially important in this account is that this subsistence change took place in a context with no appreciable increase in fertility or child survivorship, that is no population growth stimulus, and without a decrease in the availability of forage or game resources, that is no demand stress. What precipitated the change was the need for resources with particular exchange value—cattle that could be used for bridewealth. Traditionally, as foragers marrying endogamously, beehives were used in these exchanges by the Mukogodo. However, exogamous marriage of daughters resulted in a father's receiving cattle in payment from the new sons-in-law since they hailed from neighboring pastoral societies. But what had stimulated exogamous marriage in the first place? As Cronk puts it:

> Why did Mukogodo fathers prefer cattle to beehives in the first place? . . . Mukogodo men may also have realized that with a change from hunting and gathering to pastoralism their families day-to-day workload would shift from them to their wives and children. In the cave period, men were directly responsible for supplying most of the food. Livestock-owning men, in contrast, usually leave the daily herding and milking to women and children and apart from supervision of the herds and some occasional

[40] Sperling (1987) 13.

and exceptionally difficult herding, do little work around the home.[41]

This second example suggests to me that a model of pastoral adoption can be created that runs nearly on social relations alone, in this case the social-sexual politics of the distribution of work, and, in particular, the attempt by men to further exploit the labor of their wives.

As with the model of technological change driven by economic stratification, archaeological realism is potentially achievable. Barrett has argued that late prehistoric:

> Britain can be understood in terms other than those dominated by questions of technological change. Gender and age categories are renegotiated at this time by restructuring the forms of domestic space and by inventing new forms of technology concerned with food preparation and its service.[42]

While Barrett's development of the archaeological concept of 'field of discourse' from ideas of Giddens and Bourdieu is somewhat opaque, the potential for assessing the dynamic relations between men and women and the material context in which they are constructed seems real.

It may be objected that these examples do not address the core of the process of domestication since they do not deal with the pristine assumption of a new technology. Neither, of course, do the food resource models, since they do not explicitly identify a mechanism for technological discovery, a psychology of creativity. I would respond in three ways. First, the incompleteness of the archaeological record makes it unlikely in the extreme that actual original technological inspirations will be recognizably recorded in the samples we use to test our notions about innovation. Second, the complexity of pastoral tasks suggests that husbandry is not repeatedly rediscovered, it is taught. Given those admitted assertions, most of the empirical data used to examine the question of domestication in actuality will have derived from complex patterns of diffusion, to which the ethnographic vignettes offered above are specifically relevant. Third, while the ethnographic examples present instances of borrowing, the causal principles of inten-

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[41] L. Cronk (1989) 'From Hunters to Herders: Subsistence Change as a Reproductive Strategy Among the Mukogodo', *Current Anthropology* 30 (1989) 232.

[42] J.C. Barrett (1987/88) 'Fields of Discourse: Reconstituting a Social Archaeology', *Critique of Anthropology* 7 (1987/88) 14.

tionality, exchange value, economic stratification, and relations between the sexes can be reset in a pristine setting, a task that awaits solution.

At least since the work of Carleton Coon, a measure of the presence of pastoral management has been the evaluation of mortality information—the famous or infamous 'young kill' criterion.[43] A frequently offered criticism of the linkage between abundances of young in the kill and the inference of domestication is that the mortality pattern could equally have emerged from 'selective hunting' or some form of 'hunter ecology'.[44] The crucial factor which is not broached by this alternative explanation is the necessary political context which it requires. Selective hunting implies that particular age and sex categories of sighted game will be passed over by human predators in the interests of long term stability in the prey population. This is a rational approach only when there are no competitor predators—good examples are the hunting club reserves that dot the American South. Only then can one afford to bypass a target since there is confidence that the animal will not be killed by someone else. Selective hunting implies that political relations between neighboring groups exist which can establish hunting territories,[45] otherwise the 'ecological' strategy fails. The primary error is to view the adoption of pastoral techniques of game management procedures as divorced from social life, as if the adopting group was utterly isolated from external political concerns.

In sum then, elements of the post-processual radical critique associated with historical context, Marxist theory, and feminism, by focussing on intentionality, the social construction of categories, the exchange function of resources, and the internal and external political and social relations between groups, substantively advance our perception of the process of domestication and provide new approaches for tackling the zooarchaeological record of its emergence.

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[43] C. Coon (1951) *Cave Explorations in Iran, 1949* (Philadelphia: 1951). A more detailed review of the 'young kill' criterion is available in B. Hesse (1982) 'Slaughter Patterns and Domestication: The Beginnings of Pastoralism in Western Iran', *Man* n.s. 17 (1982) 403–17.

[44] D. Brothwell (1978) 'Growth and Demography: Neglected Aspects of Mammal Studies in Relation to Archaeology', *University of London Institute of Archaeology Occasional Publication* 3 (1978) 91–96.

[45] H. Hickerson (1965) 'The Virginia Deer and Intertribal Buffer Zones in the Upper Mississippi Valley', in *Man, Culture, and Animals*, eds. A. Leeds and A.P. Vayda (Washington: 1965) 43–65.

The Pig Prohibition

Over the centuries the Biblical injunction against the consumption of pig has stimulated an ocean of commentary. Much of this literature has concerned the ethical or theological implications of the legislation and is beyond the scope of this treatment. However, some work has explored for a human rationale lying behind the selection of the pig for opprobrium. It is in this arena that archaeological remains have played a role. In what follows I will attempt to summarize the problem, illustrate how some historical commentators have dealt with the issue and used ancient animal remains to support their points, and suggest how an alternative perspective, emphasizing some of the elements of both the archaeological science and post-processual approaches provide new insights into an explanation of the rule.

The Text. The legislation prohibiting the consumption of swine in the Bible is found in Leviticus 11 and Deuteronomy 14.[46]

> Lev 11.7: and the swine—although it has true hoofs, with the hoofs cleft through, it does not chew the cud: it is unclean for you.

> Deut 14.8: also the swine—for although it has true hoofs, it does not bring up the cud—is unclean for you. You shall not eat of their flesh or touch their carcasses.

What is not directly indicated by the texts themselves, since they are not couched in an historical narrative, but are part of an exposition of religious regulations, is the date these specific rules against swine were incorporated into the tradition. Scholarly opinion on this question is quite divided. For more than a century, following in the path laid out by Julius Wellhausen, Biblical research has been structured around the Documentary Hypothesis, the idea that the text was created by a complex historical process of editing.[47] It is believed that several threads of compositional and editorial tradition, called sources, successively rewove the textual fabric into the tapestry we find today in the Hebrew Bible. A determination of the precise number of sources which were responsible, the dates

[46] Citations from the Hebrew Bible are taken from *Tanakh. A New Translation of the Holy Scriptures* (Philadelphia: 1985). Citations from the Apocryphal and Deuterocanonical books are taken from the New Revised Standard Version published in *The New Oxford Annotated Bible* (New York: 1991).

[47] For a readable summary, see R.E. Friedman (1987) *Who Wrote the Bible?* (New York: 1987).

each editorial community was active, and the political setting in which each worked are subjects that have undergone constant debate and re-evaluation over the past century.[48]

While the dietary laws in Deuteronomy and Leviticus have been assigned to the Priestly Source, the date of that group's work is contested. Even the order in which Deuteronomy and Leviticus were composed is controversial. Two recent important commentaries on Leviticus take opposite positions on the matter.[49] Differences between the two versions of the food laws are significant. For instance, while in Leviticus the criteria, cud chewing and cleft hooves, that makes a land animal edible are all that is given, in Deuteronomy, an exhaustive listing of the permissible mammals is provided. These contrasts are often tied to a shift in the degree of centralization of ritual slaughter legislated by Josiah. Even more generally, the Priestly source has been placed in a number of historical contexts before, during and after the Biblically described Exile to Babylonia in the early 6th century BC. Milgrom, for instance, argues for a relatively early date for P:

> Today there is consensus that P is the repository of ancient materials, but debate still rages concering the date of its composition.... To be sure, the mounting evidence culled from the study of comparative institutions in the ancient Near East has demolished the hitherto regnant theory that P's institutions are a late Israelite creation.... Thus the diachronic study of Priestly terminology, the comparison between P and D, and the variety of data culled from realia, institutions, literary forms, and historical allusions lead inexorably to one conclusion: the Priestly texts are preexilic.... P—not just its teachings but its very texts—was composed not later than the middle of the eighth century (ca. 750 B.C.E.).[50]

At the other end of the spectrum we have Thompson, for whom the entire historical tradition of the Bible is a late composition disconnected from earlier events in and previous human

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[48] See M. Smith (1987) *Palestinian Parties and Politics that Shaped the Old Testament,* Second, corrected, edition (London: 1987).

[49] B. Levine (1989) *Leviticus, The JPS Torah Commentary* (Philadelphia: 1989), argues the more commonly accepted opinion that Leviticus presupposes Deuteronomy. However, J. Milgrom (1991) *Leviticus 1–16, A New Translation with Introduction and Commentary, The Anchor Bible* (New York: 1991) has strongly advocated the opposite position.

[50] Milgrom (1991) 3, 12, and 28. See also R.E. Friedman (1981) *The Exile and Biblical Narrative, The Formation of the Deuteronomistic and Priestly Works, Harvard Semitic Monographs* 22 (Chico CA: 1981) 145, who suggests the pig prohibition in pre-exilic.

ties to the region whose history the text claims to report. He argues that the resettlement policies that can be hypothesized for the Neo-Babylonian officials, ones which were designed to fragment the social integrity of subject peoples, make the story, as reported in the Bible, of the coordinated exile and return to Israel with politico-religious institutions more or less intact improbable.

> An understanding of the coherence of the biblical tradition, as arising out first within intellectual milieu of the Persian period, causes great difficulty in affirming the historicity of the Israel of tradition at all, and suggests rather that in dealing with this concept, the perspective of the tradition suggests we are involved with an entity that is both intellectually and literarily an entirely new creation beginning in the late Persian period's transforming revisions and collections of traditions.[51]

This approach is flatteringly endorsed by Davies who argues that three 'Israels' are present in the interpretive swirl surrounding the Bible.[52] One is 'biblical Israel'—the entity of the text, another is 'historical Israel'—the nation recognized through archaeological evidence and contemporary accounts, while the third is 'ancient Israel'—the scholarly construction of a past society that takes as its point of departure the Biblical text. Since both Thompson and Davies see serious contradictions between the historical Israel and the biblical Israel, they treat ancient Israel as a literary production containing little of historical reliability. These authors are correct that a naive intermingling of archaeological and documentary evidence in an era when historical writing was emergent[53] is dangerous and potentially misleading. However, Thompson espouses a very narrow view of historical writing[54] and a sureness in the capacity of historians to be objective. At the same time they are both remarkably confident of their ability to 'read' the archaeo-

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[51] T.L. Thompson (1992) *Early History of the Israelite People From the Written and Archaeological Sources* (Leiden: 1992) 353–54.

[52] P.R. Davies (1992) *In Search of Ancient Israel, Journal for the Study of the Old Testament, Supplement Series* 148 (Sheffield: 1992) 17.

[53] See the Introduction in B. Halpern (1988) *The First Historians, The Hebrew Bible and History* (San Francisco: 1988).

[54] Thompson (1992) 372–74, deplores the view of historiography as 'the intellectual form in which a civilization renders account to itself of its past,' a stance which he views as too loose, preferring instead an approach where 'the concepts of truth, facticity and historicity is (sic) a central, pivotal concept.' This confidence about the potential realism of historical accounts is striking in a time when objectivism is so under attack in the humanistic sciences.

logical record to produce a history for a region, Cisjordan, in a period that they assert was more or less prehistoric.[55] Archaeological data is just as susceptible to manipulation as textual sources, and the political stance that lies behind their arguments is no less obvious than the one they impute to others.

Using the limits established by the contrasting opinions of Milgrom and Thompson, the search for the origins of the pig *legislation* can be encompass a time and space framework that extends from 8th century BC Israel to greater Mesopotamia in the Persian Period. I emphasize the word legislation here, because an important issue is whether the Biblical legislation merely reinforced a practice of pig avoidance or great age or instituted a new dietary tradition.[56]

The documentary evidence that the prohibition on the pig was targeted at actual behavior is slim. Two passages from Isaiah, texts assigned to the post-exilic period, provide some basis:

> Isa. 65.3–5—The people who provoke my anger
> Who continually, to My very face
> sacrifice in gardens and burn incense on tiles;
> Who sit inside tombs
> And pass the night in secret places;
> Who eat the flesh of swine,
> With broth of unclean things in their bowls.
>
> Isa. 66.17—Those who sanctify and purify themselves to enter the groves, imitating one in the center, eating the flesh of the swine, the reptile, and the mouse, shall one and all come to an end declares—the Lord.

Ackerman argues convincingly that these passages reflect a Priestly concern with the consumption of prohibited flesh in cultic circumstances.[57] Though condemnations, they are not certain evidence that the abhorence of swine necessarily extended to less religiously charged situations. For evidence of that sort, one can turn to the Apochryphal—Deuterocanonical literature, texts deriving from the period of Persian hegemony and later. For instance in Judith, we find two references to the non-ritual observance of food laws, though not specifically

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[55] Perhaps 'telehistoric', see C. Hawkes (1954) 'Archaeological Theory and Method: Some suggestions From the Old World', *American Anthropologist* 56 (1954) 155–68.

[56] A point made by U. Hübner (1989) 'Schweine, Schweineknochen und ein Speiseverbot in alten Israel', *Vetus Testamentum* 39 (1989) 225–37.

[57] S. Ackerman (1992) *Under Every Green Tree, Popular Religion in Sixth-Century Judah* (Atlanta: 1992) 204.

the pig prohibition. In a story of heroic resistance against Holofernes, general of Nebuchadnezzar, it is reported that:

> Jth. 11.12—Since their food supply is exhausted and their water has almost given out, they have planned to kill their livestock and have determined to use all that God by his laws has forbidden them to eat.

Then, Judith, brought before Holofernes and offered food:

> Jth. 12.2—But, Judith said, 'I cannot partake of them, or it will be an offence; but I will have enough with the things I have brought with me'.

The flesh of swine figures specifically in episodes found in Maccabees. In 1 Macc. 1:47 we learn that Antiochus IV Epiphanes, a Seleucid king, desecrates the temple, insisting that pigs be sacrificed there. Macc. 1.63 reports that some 'chose to die rather than to be defiled by food.' More graphic events are reported in 2 Maccabees:

> 2 Macc. 6.18–20—Eleazar, one of the scribes in high position, a man now advanced in age and of noble presence, was being forced to open his mouth to eat swine's flesh. But he, welcoming death with honor rather than life with pollution, went up to the rack of his own accord spitting out the flesh, as all ought to go who have the courage to refuse things that it is not right to taste, even for the natural love of life.

and in 7.1–42, which tells the story of the woman and her seven sons, all of whom refused to each pork while under torture and were subsequently killed. Pigs are mentioned several times in the New Testament (Matt. 7.6, 8.30; Mark 5.11–13; Luke 8.32–33; 2 Peter 2.22) in passages that both suggest the animal was herded and held in low esteem.

One important characteristic of the texts in Leviticus, Deuteronomy and Isaiah is that the pig is mentioned in the company of other profane items. This point has been used by Mary Douglas, particularly with reference to the Preistly texts, to suggest that the polluting quality of pigs was not singular at the time the legislation in its present form entered the tradition.[58] While the avoidance of pork came to be the most visible

[58] The arguments of M. Douglas (1966) *Purity and Danger* (London: 1966) have advanced a much admired understanding of the food laws as a coherent symbolic system that does not find its origin in human practical concerns. In a subsequent publication (1975) *Implicit Meanings* (London: 1975), some important elements of the analysis are changed. See below.

component of the food laws, such was not necessarily the case at the outset. Listed with camels, hyraces, and hares, there is no hint in the text that pigs are especially hazardous. Douglas offers a classificatory approach to the logic lying behind the legislation, suggesting that the pig, along with the other three listed animals, received special legislative attention because it was anomalous within the general taxonomic scheme for classifying animals seen in the Bible, a structure based in part on principles of locomotion.

Hunn has provided an alternative rationale for coherence. He argues that there is a material as well as a symbolic basis for the animal groupings, particularly as seen in the Deuternomic listings.[59] He suggests that:

> These ten folk species constitute an exclusive and exhaustive listing of the species of the artiodactyl sub-order Ruminantia known from the home range of the Hebrews.... In short, the scientifically defined relationships of the animals cited predict the boundaries of the distinctions drawn in Deuternomy (14.3–9), as well as the priestly author / Mary Douglas rationale does.[60]

If the laws are theoretically coherent as Douglas and Hunn have argued, a piecemeal, animal by animal, examination of the problem of the food laws is unacceptable. Since the pig draws its pollution from its location in a larger symbolic system, the history of its prohibition should parallel that of all the other unclean species, listed or not. There is some reason to reject this seamless scenario. Hunn unfortunately made two errors. He used the modern fauna of the region as a reliable guide to the ancient biota. Then, he assumed that the English glosses given for the animal terms in the biblical text are certain. Such is not the case. In fact it is possible that with some of the more difficult Hebrew terms, the translators that Hunn relies on did what he has done, compared the words to a zoological guide and made a series of equations.[61] Thus perhaps some types of animals were included on the basis of a rationale other than conformation to nature. To return to Douglas, her later publication offered an enlarged perspective on the pig prohibition:

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[59] E. Hunn (1979) 'The abominations of Leviticus revisited', in *Classifications in Their Social Context*, eds. R.F. Ellen and D. Reason (London: 1979) 103–16.

[60] Hunn (1979) 106 and 113.

[61] P. Wapnish, this volume, shows how perilous a procedure this is!

> It carries the odium of multiple pollution. First it pollutes because it defies the classification of ungulates. Second, it pollutes because it eats carrion. Third it pollutes because it is reared by non-Israelites. An Israelite who betrothed a foreigner might have been liable to be offered a feast of pork. By these stages it comes plausible to represent the utterly disapproved form of sexual mating and to carry all the odium that this implies.[62]

Thus the singularity of the pig is reaffirmed and an opportunity to examine its record independent of other species provided.

Firmage provides an alternative, and more practical explanation for the coherence of the listings.[63] He argues that the priests were offering rules targeted for practical use by the faithful. The legislative objective was not to provide a glimpse of a rationale for animal classification, but to supply in the text a simple set of principles that would permit profane animals to be unambiguously segregated from those which could be slaughtered or sacrificed. The contrast is similar to the difference in zoological work between a taxonomic classification, which is intended to express evolutionary principles of descent, and a key, meant to facilitate identification.[64] Milgrom reminds us that in the Priestly key, one rule, that of cleft hoof, is sufficient to exclude almost all the profane forms and argues that inclusion of the requirement that an animal be a ruminant as well was 'deliberately formulated in order to exclude the pig, a position already espoused in Talmudic times.'[65]

Both Firmage and Milgrom therefore argue, that the negative attitude toward the pig long precedes the actual prohibiting legislation. Even if the Priestly Source is late post-exilic in composition, the attitude toward the animal may be one of the historical nuggets that even Davies admits the Bible contains.[66] The stage is thus set for a search of the archaeological record related to pig.

Many scholars have offered explanations for the negative attitude towards the pig in the Near East. Variants run the gamut from aesthetic and religious to materialist and environmental.

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[62] Douglas (1975) 79, cited by Milgrom (1989) 649.

[63] E. Firmage (1990) 'The Biblical Dietary Laws and the Concept of Holiness', *in Studies in the Pentateuch,* ed. J.A. Emerton (Leiden: 1990) 177–206.

[64] G.G. Simpson (1961) *Principles of Animal Taxonomy* (New York: 1961) 11–16.

[65] Milgrom (1989) 649.

[66] Davies (1992) 75.

Some, which invoke supernatural will or are arbitrary, cannot be evaluated. Others are simply improbable or require ways of thinking that simply were not in vogue in the ancient world. Harris has effectively discounted those explanations which suppose that an intrinsic unloveliness to the animal occasioned dislike or suggest that the pig's capacity to act as a disease vector is involved.[67] In a previous paper,[68] I classified and reviewed some of those which have archaeological implications and will not cover the same ground in detail again here.

Traditional Explanations for the Pig Prohibition and Archaeology

It is fair to say that some previous treatments have failed to use zooarchaeological information effectively or, in a few cases, have reported factual errors. For example, Hübner, in his recent review of the problem, surveyed the archaeological record for reports of pig bones or pig related artifacts.[69] Since he found them widely represented in Late Bronze Age and Iron Age contexts, he inferred that no significant differences in pig use distinguished the two periods. Armed with this summary he concluded that the Levitical injunction was a late Priestly innovation, one accomplished by an elite sector of the society for their own religious purposes. As I will show below, there is considerable interpretable variability in the pig bone record in the Bronze and Iron Age that suggests historically significant processes were in place. In another case, reliance on inexperienced opinion caused Lapp to report caches of pig bones in cultic contexts at Megiddo and Ta'annach.[70] These, he supposed, were evidence of the kind of ritual apostasy the Levitical legislators had in mind. Unfortunate for this reconstruction, a succession of zooarchaeologists have determined that the bones are not from pigs. Misidentifications have slow deaths. While Milgrom does raise the question of identification, he still mentions the bones in his commentary.[71]

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[67] M. Harris (1985) *Good to Eat, Riddles of Food and Culture* (New York: 1985) 67–71.

[68] B. Hesse (1990) 'Pig Lovers and Pig Haters: Patterns of Palestinian Pork Production', *Journal of Ethnobiology* 10 (1990) 195–225.

[69] Hübner (1989).

[70] P.W. Lapp (1964) 'The 1963 excavations at Ta'annek', *Bulletin of the American Schools of Oriental Research* 173 (1964) 35, and (1967) 'Taanach by the Waters of Megiddo,' *Biblical Archaeologist* 30 (1967) 23.

[71] Milgrom (1989) 652.

Among scholars who come to the problem from the text, explanations that are rooted in an Israelite disdain for the cultic practices of others are popular. Mention is made of Anatolian texts that indicate that pigs were used in rituals.[72] A diverse sampling of archaeological data is also often cited. DeVaux's summary of the evidence led him to conclude that swine were a part of mystery cults separate from mainstream religious thought.[73] A focus on the pig as a cult item in the religion of the cultural enemies of the Israelites is paralleled in these treatments with a tendency to view the Priestly laws themselves in terms of a religious goal.[74]

A Zooarchaeological Perspective on the Pig. What should a zooarchaeological perspective predicated on the archaeological science approach outlined above contribute to the discussion of the pig prohibition? Given its processual stance and emphasis on the human - resource relationship, an initial step would be to estimate the significance of the pig to the ancient economies of the region and link this variable to environmental considerations. In essence, this amounts to a general test of Harris' notion that pig husbandry in the Near East is strongly controlled by the distribution of moisture and suitable vegetation.[75] In my previous paper, I confirmed the utility of this principle in a review of pig bone abundances in the region more or less described by the limits of modern Israel. Here I expand the coverage with a summary of many of the zooarchaeological reports dealing with the an area encompassing Syria, Iraq, eastern Anatolia and Egypt (see Table 1). It is important to note some shortcomings of this data (references for the samples are not provided in the main text; please refer to the bibliography accompanying the table for the sources of data discussed). Sample sizes are highly variable. Collection methods used at the sites vary widely. The kinds of areas within sites selected for excavation also are diverse. In most cases, osteological proof of the domestic or wild status of the pigs is

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[72] J.D. Pritchard (1969) *Ancient Near Eastern Texts Relating to the Old Testament* (Princeton: 1969) 351.

[73] R. de Vaux (1958) 'Les Sacrifices de Porcs en Palestine et dans l'Ancient Orient', in *Vom Ugarit nach Qumrân,* eds. J. Hempel and L. Rost *Beihefte Zeitschrift für alttestamentliche Wissenshaft* 77 (Berlin: 1958) 250–65.

[74] Ackerman (1992) 210–11, argues that to the Priests, the food laws were tied to the concept of holiness and essential in maintaining the well-being of Israel; Milgrom (1989) 733, posits that the laws were intended to teach reverence for life.

[75] Harris (1985) 73–75.

not yet published. Thus, the figures supplied should only be taken as extremely rough estimates and only the widest differences taken as significant. With those caveats in mind, I believe that, on the wider scale, wetter conditions again account for some of the variability in the listings. For instance, sites in Anatolia consistently contain more pig remains than their contemporaries to the south. There is a general decrease in pig bones over time. This may be paired with the suggestion that forest cover, and with it prime pig foraging habitat, decreased as intensive agriculture increased with the expansion of large and complex polities beginning in the fourth and third millennia BC.[76]

Clearly, however, more is going on than this. Again following Harris in a processual and materialist mode, one would expect that, given the difficulty experienced in herding pigs, permanent settlements should contain pigs while transitory encampments few or none. Unfortunately, while nomadic sites are now recognized as potentially discoverable, samples from such locales have not yet been published. In a more positive vein, Crabtree's economic notion that pig husbandry is an effective first subsistence strategy[77] is congruent with the record in Philistia where the animal is associated with the first phases of settlement.

A second step in a modern zooarchaeological appraisal of the problem would be to consider how pig legislation might benefit certain whole communities within a society, a component of the post-processual approach. One component that is missing from the traditional consideration of the Priestly authors is a judgement of whether the dietary laws are related to the practical economic and political interests of the temple. Since the temple officials were operating a large redistributive institution where numbers of animals were brought for sacrifice, perhaps, as in Mesopotamian temples, they had a stake in the organization of the pastoral economy. The significance of temple activity in animal slaughter is zooarchaeologically visible. The bone remains from the altar area at Tel Dan betray patterns

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[76] H. Buitenhuis (1990) 'Archaeozoological Aspects of Late Holocene Economy and Environment in the Near East", in *Man's Role in the Shaping of the Eastern Mediterranean Landscape*, eds. S. Bottema, G. Entjes-Nieborg and W. van Zeist (Rotterdam: 1990), 201–02.

[77] P.J. Crabtree (1989) 'Sheep, Horses, Swine and Kine: A Zooarchaeological Perspective on the Anglo-Saxon Settlement of England', *Journal of Field Archaeology* 16 (1989) 205–13.

congruent with expectations drawn from textual sources.[78]

What about political control in general? Pork production has a variable history in Cisjordan. In the Middle Bronze Age, from the few reported samples a tentative pattern exists—big sites, few bones, little sites, many. In my earlier study, I linked this pattern to the proposition of Diener and Robkin that anti-pig legislation (in their case, the Islamic one) is rooted in the objective of urban elites to control the economy. They argue that this goal is met though dissuading subjects from engaging in subsistence pursuits, in this case pig husbandry, that are not easily exploited by central authorities.[79] The fact that swine husbandry almost dissappeared in the Levant at the time of Assyrian invasions may be related to a policy by those facing the attack of depopulating the countryside and concentrating citizenry in the cities for defensive purposes. However, the rural-urban dichotomy in the use of pigs is not confirmed in the wider regional sample. In fact, at the site of Korucutepe, pig remains increase with the political significance of the community. Lidar, another Anatolian site, also contains more abundant pig remains than the smaller communities in its environs. Most striking, though it is a small sample, is the abundance of pig in the large city of Isin in southern Iraq.

A third step in examining the pig question from a zooarchaeological perspective would be to test the notion that pig use was a measure of social distinction within a community. Here documentary support is available. For Egypt we have the report of Herodotus that:

> The pig is regarded among them as an unclean animal, so much that if a man in passing accidently touch a pig, he instantly hurries to the river, and plunges in whith allhis clothes on. Hence, two, the swineherds, notwithstanding that they are of pure Egyptian blood, are forbidden to enter into any of the temples, which are open to all other Egyptians; and further, no one will give his daughter in marriage to a swineherd, or take a wife from among them. . . .[80]

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[78] P. Wapnish and B. Hesse (1991) 'Faunal Remains From Tel Dan: Perspectives On Animal Production at a Village, Urban and Ritual Center', *ArchæoZoologia* 4 (1991) 9–86.

[79] P. Diener and E.E. Robkin (1978) 'Ecology, Evolution and the Search for Cultural Origins: The Question of Islamic Pig Prohibition', *Current Anthropology* 19 (1978) 493–540.

[80] Herodotus, II.47.

For Mesopotamia, there are texts which indicate the animal was held in low regard. Milgrom cites an Assyrian tablet dated to 716 BC which reads:

> The pig is unholy [. . .] bespattering his backside, making the streets smell, polluting the houses. The pig is not fit for a temple, lacks sense, is not allowed to tread on pavements, an abomination to all the gods. . . .[81]

There is zooarchaeological evidence that these attitudes are of considerable antiquity. In Egypt at Elaphantine pig bones are found in household / 'secular' contexts but not in the temple areas. At Amarna, the large sample of pig bones is found in the workers' quarters, other sectors of the site produced few. In the Jordan Valley, at Tell el-Hayyat in the early Middle Bronze Age, the pattern is the same as found at Elephantine.[82] Finally, at al-Hiba (Lagash) in southern Mesopotamia, the same disassociation of sacred space and pig bones is found. In Cisjordan, evidence from Iron Age Batash (Timna) suggests that areas with and without pig remains can be found.

These summary zooarchaeological observations suggest that a number of intertwined factors can explain much of the variability in the use of pigs in the ancient Near East. However, some residuals are left. The broadest change, that of the sharp drop from third millennium to second millennium levels all across the region remains unaccounted. The abundance of pig remains in a few urban centers is likewise unexplained unless the accidents of excavation have produced samples from the debris of lower socio-economic classes. None of the factors provides a proximate cause for the anti-pig legislation in the Bible. As mentioned above, processual accounts are not designed to explain events. However, they do provide a structure for understanding the system in which the decision to prohibit was taken.

Conclusion

This discussion set out to consider the viability of zooarchaeology in the Near East in the intellectual climate that has grown up in the 80's and 90's, and its persistent position in historical

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81 Milgrom (1989) 650.
82 M. Metzger, personal communication.

period research. A review of the two problems, one each of prehistory and history, suggested that zooarchaeology does need to emulate the chameleon. It can adopt perspectives common to historical periods and apply them successfully to prehistoric considerations of processual change. Conversely, in working in historical periods, zooarchaeology needs to surrender its developed processual approach. A consideration of underlying systemic structure contributes to an understanding of historical decisions. Perhaps the fact that zooarchaeologists are defined by the material they study is in the end, the best strategy, permitting as it does, an enormous flexibility in responding to the range of research problems to be faced in the study of the ancient Near East.

Table 1. Data on pig bone abundance in sites outside of the southern Levant. Unless otherwise indicated the abundance is measured by the percentage of identified bones registered as *Sus*. Sites are arranged in chronological order in rough geographical regions. References to the literature are coded within parentheses following the site.

Site (Reference)	*Location*	*Date*	*Context*	*%Pig*
Omari (10)	Egypt	Neolithic	Village	36
Merimde (6)	Egypt	Neolithic	Village	39
Heirakonpolis (34)	Egypt	Predyn		41
Maadi (12)	Egypt	Predyn	Village	20
Ibrahim Awad (8)	Egypt	NaqIIc-2900		47
Kom el-Hisn (34)	Egypt	Old King	Village	51
Giza (34)	Egypt	Old King		1
el-Dab'a (34)	Egypt	13th Dyn		5
Amarna (22)	Egypt	New King		47
El-Hibeh (34)	Egypt	New King		7
Elephantine (24)	Egypt	1–4 Dyn	Satet Temple	<1
"	"	Old King	"	2
"	"	Mid King	"	2 (small sample)
"	"	New King	"	0 (small sample)
"	"	MK-2IP	House	20
"	"	Old King	SE-City	12
"	"	1IP-MK	SW	31
"	"	Old King	Museum	14
"	"	Old King	Stone wkshop	<1
"	"	Old King	South-City	2
Girikihaciyan (29)	Turkey	Halaf	Village	17
Turlu (20)	Turkey	Halaf		26
Hassek (35)	Turkey	EB		51
Hayaz (17)	Turkey	EB	Small	10
Yarikkaya (14)	Turkey	EB	Small	33
Gritille (36)	Turkey	EB	Small	18

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Table 1. continued

Site (Reference)	*Location*	*Date*	*Context*	*%Pig*
Lidar (26)	Turkey	EB	Town	12
"	"	MB	"	20
"	"	LB	"	18
"	"	6–400	"	20
Korucutepe (5)	Turkey	EB	Village	7
"	"	MB	Fort. Town	11
"	"	LB	Imperial Center	13
"	"	Iron	Village	1
Shams ed-Din (20)	Syria	Halaf		3
Leilan (28)	Syria	EB	Urban	abundant
Apamée (21)	Syria	EB-early		37
"	"	EB-late		4
Sweyhat (16)	Syria	EB-late	Fort. Town	<1
Hadidi (18)	Syria	BA	Urban	2
Halawa (9)	Syria	27–2500	Temple/House	<1
"	"	23–2000	House	<1
"	"	"	Temple	<1
Yarim (20)	Iraq	Halaf		12
Arpachiyah (20)	Iraq	Halaf		14
Banahilk (20)	Iraq	Halaf		16
Munbāqa (7)	Iraq	mid2nd cBC		<1
Rubeidheh (32)	E. Iraq	Uruk		0
Razuk (4)	E. Iraq	28–2700		10
Abqa' (1)	E. Iraq	25–2000	Town	30 (+Pig deposit)
Atiqeh (4)	E. Iraq	24–2200		16
Uruk (11)	S. Iraq	Uruk IV		1
"	"	ED		1
Lagash (31)	S. Iraq	26–2350	Admin. Resid.	16
"	"	"	Temple	8
Abu Salabikh (19)	S. Iraq	ED		2nd after S/G
Asmar (23)	S. Iraq	ED		27
Abu Duwari (27)	S. Iraq	20–1500	Admin. Bldg.	20 (Small sample)
Nippur (3)	S. Iraq	20–1500	House	22
Isin (13)	S. Iraq	20–1500		18
Uruk (11)	S. Iraq	1900	Temple	<1
Isin (15)	S. Iraq	OB		31 (Small sample)
" (2)		11–800		3
Uruk (11)	S. Iraq	1100–600		<1
Isin (2)	S. Iraq	8–700		14 (Small sample)

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TOWARDS ESTABLISHING A CONCEPTUAL BASIS FOR ANIMAL CATEGORIES IN ARCHAEOLOGY

PAULA WAPNISH

INTRODUCTION

The study of animal bones from archaeological sites has traditionally been framed in a zoological perspective. The first step has always been to identify the taxa present, then how (and where) they are represented, abnormalities, and so on. This is as it should be, given that the main objective of such research is to describe ancient animal procurement or production and to aid environmental reconstructions of the past, in other words, analyses predicated on scientific categories. Having begun in the idiom of Western science, few investigators move to the more subjective, some might say retrogressive, realm of 'folk science.' Paradoxically, however, this is the method most likely to take us beyond a nuts and bolts, purely materialist approach to animal bone questions and contribute more effectively to historical research agendas.

We cannot hope to achieve both a broader look into past human and animal interactions as well as a finer appreciation of a zoocultural system if we consider animals only as Linnaean markers. History, with its textual materials, provides the opportunity to explore culturally conditioned animal categories and recast the analytic categories used to approach osteological materials. The difficulty is that this is not straightforward. A folk taxonomic perspective must mediate between the zoological categories, the ancient terms, and the bones we recover.

We know that animals function in many cultural domains, from the basics of subsistence to complex symbols woven throughout a society. A symbolic or totemic animal, while represented by a 'real' animal, may in fact be defined by characteristics and associations that have nothing to do with the animal itself. Similarly, a feature intrinsic to the real animal may have served as the basis of or linked it to an association from which a symbolic dimension developed that went on to overshadow and finally obscure the original real animal motivation.

Archaeozoologists have seldom concerned themselves with these 'animals' since they lie outside the objective and quantifiable province of their goals. Nevertheless, their potential to inform on a zoocultural system is considerable.

Closer to home, but still ignored by the archaeozoologist, are the folk animal categories that appear to resemble the scientific animal categories of what are presumed to be the same animals. The 'real world' features that delineate the animal categories in each system may overlap to an appreciable degree, but the differences between them are profound and significant for zooarchaeological research. Even when an animal devolves to the components of meat, milk, fiber, labor, and other by-products which the modern researcher must investigate through the scientific referent, the ancient conception of that animal may more closely approximate the modern folk definition than the scientific one.

This is a general problem long recognized by lexicographers under a different guise, that of analytic versus descriptive definition. Taking an animal name as an example highlights the scientific/folk difference. In analytic definition an object is identified to a known class and its place within that class is stated. The analytic definition of cow would be 'the mature female of domestic cattle (genus *Bos*).' Taking this a step further would reveal that the genus *Bos* belongs to the family Bovidae, 'ruminants distinguished by a polycotyledonary placenta; by nondeciduous, hollow unbranched horns, and usually by the presence of a gall bladder. . .'[1] Few people uninvolved with meat production or dairying would know this. They'd tell you that a cow was a large four-legged animal with horns that gives milk, makes a mooing sound, whose coat color may be rust and white (Hereford), black (Angus), cream (Charolais)—to name a few common American varieties—and so on. In other words, they are describing how a cow appears to the senses, or its descriptive definition. Lexicographers find that the preciseness of analytic description may still be insufficient for a complete understanding of an object. It often benefits from the addition of a descriptive definition which aims for non-specialist recognition.

Compounding the problem is the fact that archaeozoologists are not the only specialists wedded to Linnaean categories.

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[1] C. Laird (1978) 'Language and the Dictionary', *Webster's New World Dictionary of the American Language*, 2nd ed. (1978) XVXXX.

Animal names in ancient texts have traditionally been identified with reference to zoology as well. A good example is philologists working with cuneiform sources from the ancient Near East. Either they worked only with texts, ignoring all but language-related data, or they used contextual information providing clues about behavior or morphology to match with a modern animal identified in a taxonomic classification. While this was a proper route to identification, few philologists understood modern zoological classification well enough to use it appropriately. Most 'overidentified' animals, equating the ancient term with a Latin binomial that indicated species when an identification at that level may not have best approximated the folk unit. Some went so far as to give the trinomial of a modern subspecific identification.[2]

Textual sources often do not give enough information to make an identification to species level.[3] Philologists (as well as historians and art historians) seem to have a pervasive resistance to generic identification, probably because they have been always looking for exotica in the faunal record. Often, however, the scientific genus will more closely approximate the conceptual configuration of the folk taxon than the scientific species. So even if the archaeozoologist identifies bones to species, enabling him to investigate questions of production and environment in a scientific framework, how those animals categories 'mean' in the culture, how they intersect with the scientific category, and how they shape manipulation of real animals, are questions that philologists and archaeozoologists must address through folk classification. With regard to animals of the past, these two sets of scholars necessarily provide information for each other. Archaeozoologists depend, in part, on the translation of animal names to learn about where animals lived, how they were raised, hunted, etc. Philologists especially need information about ancient species distributions to make cogent choices for translation. Folk classification principles can finetune this dialogue by helping to reveal categories as the ancients' perceived them rather than as modern constructs.

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[2] The instability of populations below the species level means that identifying ancient animal bones or names with modern subspecies is chasing the wind.

[3] Certainly anyone who has worked with European or near eastern faunal collections is well aware that bones often defy identification to species level. The most common 'taxon' found, the sheep/goat, is of course not a taxon at all, but an indication that the bone belonged to an animal from one of two genera, *Ovis* or *Capra*.

The Theoretical Basis of Folk Classification[4]

The formal study of folk classification grew out of the 1950's movement in American anthropology called 'The New Ethnography.'[5]Also known as ethnoscience, formal semantic analysis, ethnolinguistics, and cognitive anthropology, the goal was to discover 'how different peoples organize and use their cultures . . . an attempt to understand the *organizing principles* underlying behavior.'[6] Cognitive models based on adequate information were judged capable of predicting behavior. These models were sought in lexical domains, because the native's universe was thought to be embodied in his lexicon.[7] Cognitive anthropologists investigated various domains and their relation to broader cultural patterns by looking at how objects were named and how those names were grouped into larger units. 'Naming is seen as one of the chief methods for imposing order on perception. . . . By naming we classify and put objects which are similar into the same category, even though we can perceive differences among them.'[8]

Investigators soon began to focus on ethnobiological lexicons because they constituted domains of 'real' objects. A plant or animal name signified, above all, an existing unit in nature. The similarities and differences between native and scientific biological classification had not gone unnoticed in earlier fieldwork. Now anthropologists hoped that systematic inquiries of these phenomena would result in explanations bearing on universal aspects of human cognition. In the 1960's and 1970's Berlin and his associates[9] investigated native categories of na-

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[4] The discussion found here and in the next section is based on my doctoral dissertation, (1985) 'Animal Names and Animal Classifications in Mesopotamia: An Interdisciplinary Approach Based on Folk Taxonomy' (Unpublished Ph.D. Dissertation, Columbia University, 1985).

[5] The term was proposed by W. Sturtevant (1964) 'Studies in Ethnoscience', *American Anthropologist* 66 (1964) 99–131; see also, M. Harris (1969) *The Rise of Anthropological Theory: A History of Theories of Culture* (London: 1969) 568.

[6] S.A. Tyler (1969) 'Introduction', in *Cognitive Anthropology*, ed. S.A. Tyler (New York: 1969) 4.

[7] O. Werner (1972) 'Ethnoscience', *Annual Review of Anthropology* 1 (1972) 278.

[8] Werner (1972) 276.

[9] Two of the earliest explications of their method and goals are found in B. Berlin, D.E. Breedlove, and P.H. Raven, (1966) 'Folk Taxonomies and Biological Classification', *Science* 154 (1966) 273–75, and their (1973) 'General Principles of Classification and Nomenclature in Folk Biology', *American Anthropologist* 75 (1973) 214–42. The latter is the classic in the field. See also their study (1974) *Principles of Tzeltal Plant Classification: An Introduction to the Botanical Ethnography of a Mayan-Speaking Community of Highland Chiapas* (New York: 1974).

ture by generating highly operationalized, formal classificatory models based on rigorous linguistic techniques originally devised to describe phonology and grammar in native languages. Berlin, his co-workers and students, formed the core of what came to be known as the 'formalist' branch of ethnobiology. They assumed that discrete domains comprised the whole of any culture, characterizing culture as a 'union of all individual competences'.[10] Thus, they were concerned with devising analytic tools to reveal the content, organization, boundaries and interaction of these in the larger socio-cultural picture.

Mechanistic models that promoted the first three of these areas at the expense of the last provoked a great deal of criticism. More sociologically oriented anthropologists railed against assuming the existence of discrete domains, and pointed out that many domains may be highly variable and amibiguous across a culture yet exert considerable influence on knowledge and behavior.[11] Within a culture, a single domain could be partitioned in numerous ways, for example, as a constituent segment in classifications of domains more inclusive than itself. Further, within any single classificatory frame some parts of a domain could be more highly structured than others, resulting in lexical expression of more or less ambiguity.

While ethnobiologists embracing formalist theory did not deny the existence of variability in the classification of a single domain, they held that empirical, descriptive categories were basic to all classificatory schemes. Variations and divergences were seen to be logically patterned and therefore explainable. This theoretical starting point derived and drew sustenance from cognitive psychology, a subdiscipline of American psychology which, beginning in the 1950's, challenged the previously held notion that the world was divided arbitrarily.[12] Earlier research had focused on the relationship between

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[10] O. Werner (1969) 'The Basic Assumptions of Ethnoscience', *Semiotica* 1 (1969) 333.

[11] See, for example, Harris (1969) 583; M. Crick (1976) *Explorations in Language and Meaning*, (New York: 1976) 72. R.F. Ellen (1979) 'Introductory Essay', in *Classifications in Their Social Context*, eds. R.F. Ellen and D. Reason (London: 1979) 1–17, provides an excellent discussion of variation in classification.

[12] The influence that cognitive psychology had on cognitive anthropology, and thus on folk classification is detailed by the Laboratory of Comparative Human Cognition, The Rockefeller University (1978) 'Cognition as a Residual Category in Anthropology', *Annual Review of Anthropology* 7 (1978) 51–69. The critical groundwork for cognitive studies was J.S. Bruner, J. Goodnow, and G. Austin (1956) *A study of Thinking* (New York: 1956).

stimulus and response as arbitrary systems. Cognitive studies recognized that categories, especially of real world objects, are formed via the interaction between stimulus and process. The configurations of real world objects could not be overlooked in the shaping of mental images.[13]

Rosch's work sought to explain how categories were encoded in the native language of a particular culture at a given point in time. She proposed two basic principles to facilitate this. *Cognitive economy* is the rationale behind category systems whose purpose 'is to provide maximum information with the least cognitive effort.'[14] *Perceived world structure* is the idea that real world objects possess a high correlation of features rather than arbitrary attributes, thereby providing stimuli in the form of structured information. These principles predict that cognitive efficiency will result if categories closely describe real world structures,[15] a position that measurably influenced folk classification.

Ancient Texts and Folk Classification

Initially it would seem that the formal study of lexical fields from sources as disparate as non-literate native informants and ancient written texts have no basis for interaction. But the positivist perspective as applied through folk classification is critical for informing translations of ancient animal terms. The corpus of cuneiform literature contains a body of knowledge about a term that does not necessarily express all of the knowledge possessed by the culture regarding a particular animal. The philologist, confronted by an animal term and all its attendant facts, often has only indirect access to what motivated the cognitive processes that established the word's meaning. The investigator of spoken languages is more fortunate, because a greater potential exists to specify the empirical or more purely cultural mediators of cognition. In a typical field situation the ethnographer shows his informants some pictures or specimens of animals to which they assign labels. Additional questioning is usually structured to reveal groupings, their basis and relationship to each other, and the functional relevance of the animal in question. The ethnographer then weighs the infor-

[13] E. Rosch and B.B. Lloyd (1978) 'Introduction', in *Cognition and Categorization*, eds. E. Rosch and B.B. Lloyd (Hillsdale NJ: 1978) 1–13.
[14] Rosch in Rosch and Lloyd (1978) 28.
[15] Rosch in Rosch and Lloyd (1978) 12.

mant's responses with his own observational data to describe the folk classification or, at the very least, some of the categories involved. Compared to the philologist, when confronted by terms of reference the ethnographer has more direct access to: 1) the objects they denote; 2) their extension, or the group of objects to which the term is applied—we may call this identification; 3) their intension, or the set or properties criterial for defining the category denoted by the term of reference—we may call this classification.

Therefore, by careful questioning of a variety of informants the ethnographer can learn the concept underlying a term of reference since by definition this includes all of the knowledge (both intensional and extensional) pertaining to a category of objects denoted by a particular term of reference.[16] Because he is dealing with a known referent, the ethnographer can more readily judge if and how intensional knowledge is based on empirical observation or cultural factors.

On the other hand, in western philosophy, a long tradition of exploring the concept of linguistic meaning in the context of reality has produced diverse interpretations of the relationships involved. In the last several decades Stephen Ullman, a noted figure in semantic studies, expounded a definition of meaning markedly different from the one operationalized by practitioners of folk classification. His position has largely held sway in philological studies. Ullman modified the tripartite relationship which the famous study by Ogden and Richards[17] schematicized (below) between words, thoughts and things:

```
        thought or reference
          /              \
symbol  ——————————  referent
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He recast the triangle somewhat differently in another treatise as:

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          sense
        /       \
name   ———————   thing
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[16] The processes involved in cognition mentioned here are more fully discussed by J.M. Anglin (1977) *Word, Object, and Conceptual Development* (New York: 1977) 27–29.

[17] C.K. Ogden and I.A. Richards (1946) *The Meaning of Meaning. A Study of the Influence of Language and Thought Upon Thought and the Science of Symbolism* (London: 1946). This is the scheme repeated by S. Ullman in (1964) *Language and Style* (New York: 1964) 18.

Like the medieval philosophy he cites, Ullman recognizes no direct relationship between 'name' and 'thing,' being most influenced in this respect by the work of Ogden and Richards. The latter's linguistic philosophy conceives of thinking as a kind of sign interpretation based upon instantiated empirical experience between speaker and referent.[18] An individual's recognition of a sign results from successful application in various contexts. This ultimately means that 'The relationship between symbol and referent is scientifically inexplicable; it is interpreted by mediation of the reference-action.'[19]

Having described the imprinted connection (or lack thereof) between the 'name'—'thing' axis, we may turn to Ullman's explanation of the other relationships in the triangle:

> The diagram offers too little and too much to the linguist. On the one hand, he can safely dispense with the 'thing' and confine his attention to the other two terms; he is not concerned with the non-linguistic world as such, only with those aspects of it which are relevant to and embodied in language.[20]

Ullman is not concerned with the 'thing,' because, as he states, 'all linguistically relevant features of the referent will enter *via* the sense, into the constitution of the word and will thus come within the purview of semantic analysis.'[21] From a purely linguistic view the 'thought' or 'sense' *is* the 'thing' for all practical purposes. A referent may not change over time, but if the conception of it does, language activity responds only to the changed awareness.

This approach does not aid the philologist in the pursuit of language-culture study. As a translator he must deduce as accurate an equivalent as possible for the textual referent from a 'sense' often incomplete. Understanding the relationship between name and referent, not just its linguistic concept, is desirable and necessary if culture is viewed as more than a series of ideas but includes pan-human capabilities as well. Ullman's definition of meaning, which assumes a knowledge

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[18] P. Hartmann and S.J. Schmidt (1972) 'Theory and Philosophy of Language', in *Current Trends in Linguistics*, Vol. 9, (*Linguistics in Western Europe*) ed. T.A. Sebeok (The Hague: 1972) 3–81, esp. 19–20.

[19] Hartmann and Schmidt (1972) 20. They note further, that Ogden and Richards used sign and symbol interchangeably, whereas de Saussure reserved sign for linguistic construal alone. Like Ogden and Richards, de Saussure believed that signs were arbitrary, but unlike them he distinguished symbols because they manifest a natural connection to their referents.

[20] Ullman (1964) 18.

[21] Ullman (1964) 19.

of the entire 'sense' of a 'thing' in the presence of a known referent, is insufficient for the investigator of dead languages because the data of the 'sense' is often incomplete or skewed. Therefore, identification of the referent need not necessarily follow from the 'sense.' In the assertion that linguistic meaning is manifest only in the dynamic of the relationship between 'name' and 'sense' Ullmann relegates investigation of the connection between 'sense' and 'thing' to the realm of psychology. While he acknowledges the role of language in the formation of mental concepts, he does not inquire how or if the referent bears on conceptualization—this being the purview of the epistomologist. The philologist, concerned only with language, 'is in a position to confine himself to one of the three (two real and one imputed) dyadic relationships.....'[22] This view of meaning is incomplete in light of interdisciplinary studies which articulate the role of language and real world objects in cognition and because it isolates language from its cultural matrix in the method of application. Since all thought is not given linguistic expression, the absence of construal as a unit of language does not predict conceptual absence in a particular culture. That linguistic relevance is informed by more than linguistic phenomena has been demonstrated repeatedly in ethnobiological studies across various cultures. Investigators often uncover the existence of covert categories, sets which are clearly conceptualized and function in the ordering of the native's classification, but which are unlabelled.[23] Were 'names' and 'senses' investigated only as discrete linguistic features outside of their cultural domains, this dimension of conceptualization would be lost as would its impact on behavior.

Lexicographic mistakes in the translation of animal terms in cuneiform texts have usually resulted from the failure to treat the 'sense'—'thing' relationship rigorously. Ethnobiologists have focussed their work to examine this relationship—the processes called extension or category application and intension

[22] Ullman (1964) 72.

[23] Investigators have documented covert categories in many cultures. One of the earliest was Harold Conklin in his study of folkbotany among the Hanunoo of the Philippines (1954) 'The Relation of Hanunoo Culture t6 the Plant World', Ph.D. dissertation, Yale University, 1954. The formal elicitation techniques operationalized by Berlin, et al., established a methodology for discovering unlabelled native groupings, see B. Berlin (1968) 'Covert Categories and Folk Taxonomies', *American Anthropologist* 70 (1968) 214–42., and B. Berlin (1974) 'Further Notes on Covert Categories', *American Anthropologist* 76 (1974) 327–31.

or category definition—in light of the cognitive stimuli associated with real world objects (plants and animals). Folk taxonomic models are a typology of folk taxa based on psychological criteria that map patterns of nomenclature with principles of classification. They can aid the philologist in a number of ways, most notably through the use of nomenclatural patterns in discrete domains that correspond to general conceptual categories. When faced with ambiguous or insufficient data concerning a referent, lexical encoding can help to make a lexical decision. Werner notes that 'At present practical ethnoscience leads to better lexicography and better translation.'[24]

Non-Formalist Ethnobiology

It was noted above that in practice ethnobiology was divided into two (broad) theoretical camps, the formalists and the more sociologically oriented researchers who were not persuaded that categorization and lexical encoding were primarily motivated by empirical stimuli. British folk studies, for example, have traditionally focused on symbolic referents in language and society. Many American researchers, having initially begun ethnobiological fieldwork with a formalist perspective, came to the conclusion that while objective considerations influence cognition in some domains, culture-specific mediators are too pervasive to be assigned secondary status in this process. The primacy or interdependence of objective/subjective semantic indices had to be investigated for each case before concluding that perceptual knowledge took precedence over ideology in the processes of cognition and categorization.[25]

Not just objects and their categorization, the so-called building blocks of knowledge, but whole and /or partial classifications were found to be variably motivated as well. Classifications and their parts can be built and ordered empirically, functionally, symbolically (i.e., totemic arrangements), etc., or by a combination of motivators. For example, in the Mesopotamian lexical series *Har-ra=hubullu* (table XIII enumerating domestic livestock)[26] only the most important domestic animal groups are treated (sheep, goats, cattle and equid) and they are listed by subcategories of gender and age. Each subcategory is further

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[24] Werner (1972) 303.
[25] For discussion and references to these topics see Wapnish (1985) 86–106.
[26] B. Landsberger (1960) *Materialien Zum Sumerischen Lexikon* VIII/I, *The Fauna of Ancient Mesopotamia, First Part, Tablet XIII,* (Rome: 1960).

described by various attributes such as color, geography, type of feed, disease, or stage in the reproductive, weaning or shearing cycle, factors of primary importance in production. Also of economic import in an expanded sense are animals described as designates for particular temple offerings, ceremonies or gifts, animals for deposits, dowries or a certain group of workers. Of a more prosaic nature are descriptions of braying, strength, and gnawing, although these qualities (certainly strength) might interest the herdsman.

This is a good example of a 'special purpose classification,' one that is functionally motivated. The nature of the information contained in this list is comprehensive within the context of domestic production. Yet the comprehensiveness goes beyond functional concerns with several entries for animals used only in mythological contexts. These 'intrusions' from literary texts point to a mixing of classifications, the interdigitation of (whole/part) classifications of different motivation. The history of folk classifications has shown that it is not unusual for mythological creatures to be included with those palpably real. Mesopotamians, like other peoples, had little difficulty reconciling creatures from different dimensions.[27] Accordingly, nonformalist proponents of folk biology stress the role that context plays in the configuration and ordering of different domains and their classification.

The formalists, led by Berlin and his school, not only maintained that categories and their arrangements were shaped by 'what is out there' but that this was the primary, or logically prior cognition upon which all other conceptualization was built. They called the cross-cultural proclivity of humans to delimit and organize their surrounding biological world irrespective of utilitarian or other cultural concerns 'general purpose classification.'[28] This formed the core of knowledge allowing for more purposeful arrangements. Needless to say, with researchers documenting empirical and ideological, general

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[27] Such mixing of classifications is not limited to folk biology but occurs in scientific classification as well, Aristotle's work being a familiar case in point. Yet as late as the 17th century the naturalist Aldrovandi interwove precise anatomical description, hearsay, fable, mythologic references, plus heraldic, and medicinal uses, into his scientific treatises. Ellen (1979) 29 n. 5, comments that 'Schemes such as Aldronvandi's do not represent bad science but part of rational, ideological constructions of knowledge for the societies that produced them.'

[28] Berlin, Breedlove, and Raven (1966) (1968) (1973) (1974) passim, discuss the differences between general and special purpose classification.

and special purpose classifications in native cultures, an impasse was reached regarding the form and motivation underlying pan human cognition of biological domains. Additional studies continued to provide further insights but, on balance, the newer evidence provided no basis for reconciling the disparate results. However, a 1990 study by Scott Atran[29] comes down on the side of the formalists, though not without clarification and rejection of some previously held notions.

Formalist Principles Recast

Atran's book is a penetrating work demonstrating how the sciences of modern zoology and botany emerged from the common-sense of folkbiological knowledge. Calling upon philosophy, systematics and cognitive anthropology, he argues that cross-cultural parallels in partitioning of the biological world occur only through common-sense perceptual distinctions which recognize morphological patterns of similarity and difference among groups of organisms. '. . . my aim is to show how our universally held conception of the living world is both historically prior to, and psychologically necessary for, any scientific— or symbolic—elaboration of that world. As we shall see, ordinary notions of species (generic-specieme), family (covert fragment) and class (life-form) not only continue to provide epistemic access to our usual environment, but also cognitive access to the biological universe at large.'[30]

Atran presents a convincing argument that the human brain organizes the domain of living kinds differently from that of other natural objects. This results, cross-culturally, in transitively structured taxonomies that are uncharacteristic of other domains.[31] Folk classification, then, is a taxonomy,[32] rigidly structured in terms of (not less than) three hierarchical levels (unique beginner, generic-specieme, and life-form). Note that what is claimed is the universal occurrence of taxonomic categories (ranked classes of taxa) not taxa (groups of organ-

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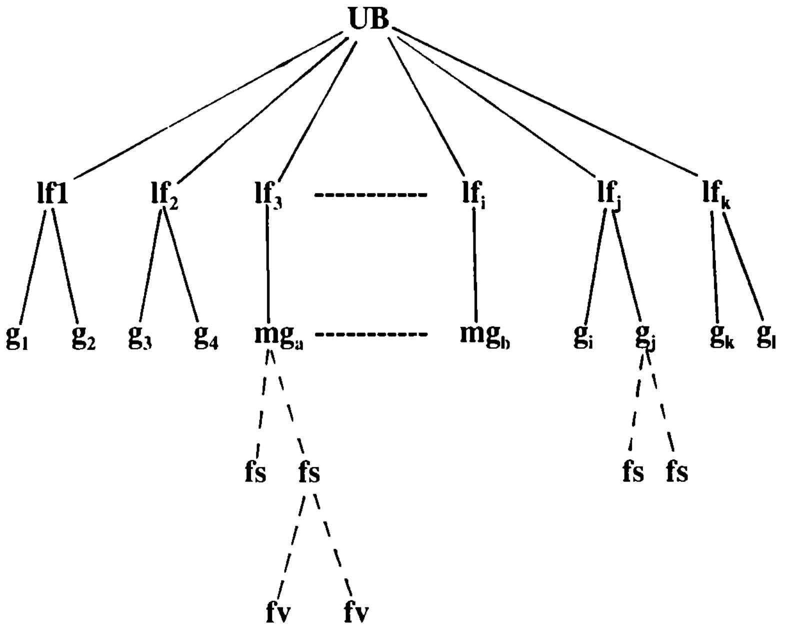
[29] S. Atran (1990) *Cognitive Foundations of Natural History. Towards an Anthropology of Science* (Cambridge: 1990).

[30] Atran (1990) 13.

[31] Atran (1990) chapter 3, 'The Semantics of Living Kinds'.

[32] Atran (1990) 52–3, points out that 'Neither folkbiological systems nor the Linnaean system fit the usual definition of "taxonomy" given in the psychological and anthropological literature. In fact, it is difficult to imagine any naturally non-arbitrary order of groups that fit.' In recognizing this, Atran circumvents the problem that earlier investigators had with the logical arrangement of folk taxa into ranked categories.



UB — Unique Beginner
lf — life-form
mg — monogeneric life-form
g — generic-specieme
fs — folkspecific
fv — folk varietal

Figure 1. Relationship between universal ethnobiological categories following the illustration provided by Atran (1990), figure 1, which is itself modified from Berlin, Breedlove, and Raven (1973).

isms). Figure 1 presents Atran's[33] reformulation of the schematic relationship of ethnobiological categories originally proposed by Berlin, et al.[34] It may be summarized thus:

1. Every natural object is either a living kind or not.
2. Every living kind is either a plant or an animal.
3. Each plant or animal belongs to one and only one basic taxon, GS.
4. No two basic taxa share all of their characteristic phenomenal properties, that is, for all GS_i and for any GS_j, GS_j

[33] Atran (1990) 56.
[34] Berlin, Breedlove, and Raven (1973) fig. 1, and p. 215.

lacks at least one readily perceptible feature characteristic of GS_i.

5. Every basic taxon GS, belongs to only one life-form taxon, LF.

6. For every LF_i, there is at least one phenomenal property, D_i, which is characteristic and diagnostic in the sense that for every LF_j, D_i is not characteristic.

The category *unique beginner* roughly correlates with 'kingdom' in scientific classification. It is not always named and often difficult to determine its inclusive and restrictive dimensions. A single label, like 'beast' or a descriptive phrase may mark the unique beginner. Despite the absence of lexical encoding in some languages, cross-cultural studies reveal that from early childhood humans conceptually distinguish plants from animals.

Classification begins by sorting organisms into basic level-taxa. In natural languages the basic level of abstraction is the concept of an object most functional for naming or thinking about that object in a variety of situations. Basic level objects appear to be at once the most abstract and inclusive categories for which it is 'possible to form a mental image isomorphic to the appearance of members of the class as a whole.'[35] In zoology, Simpson recognized that 'the basic or one might say primitive step in classification' involves 'non-technical recognition—identification—' which is 'normally not by separate characters but by a mental image of the whole animal.'[36] Berlin and his associates demonstrated that folk classifications also began with conceptualizations of taxa at basic-level, being the first taxa lexically encoded in all languages.[37] Basic-level constitutes the rank at which organisms are morphologically most distinct.[38]

Berlin called (folk) taxa of this rank *generic*, citing a long historical precedent for the term as applied to biological categories whose discontinuities are readily perceived.[39] Atran's re-

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[35] Rosch in Rosch and Lloyd (1978) 34.

[36] G.G. Simpson (1961) *Principles of Animal Taxonomy* (New York: 1961) 12. Similarly, A. Cronquist (1968) *The Evolution and Classification of Flowering Plants* (New York: 1968) 30, a plant taxonomist, wrote '. . . we try to define general in such a way that one can recognize a genus from its aspect, without recourse to technical characters not readily visible to the naked eye'.

[37] Brent Berlin proposed this for plants in (1972) 'Speculations on the Growth of Ethnobotanical Nomenclature', *Language and Society* 1 (1972): 63–98. Berlin, Breedlove, and Raven (1973) showed it held for animals as well.

[38] Berlin, Breedlove, and Raven (1973); see also, Brent Berlin, (1978) 'Ethnobiological Classification', in Rosch and Lloyd (1978) 9–26.

[39] Berlin in Rosch and Lloyd (1978) 14–17; B. Berlin, J.S. Boster, and J.P. O'Neill (1981) 'The Perceptual Basis of Ethnobiological Classification:

working of the relevant ethnobiological data led him to conclude that there is little conceptual difference between folk taxa of generic rank and the rank of species (Berlin's 'folk species'):

> In sum, within any given local community a principled distinction between genus and species is usually impossible perceptually because most genera are monospecific. The distinction is frequently also irrelevant conceptually inasmuch as the species of locally monogeneric or minimally polytypic families may have the same sorts of morphological, geographical and behavioral correlates as the genera of locally polygeneric families. In such cases, the perceptible morpo-ecological distance between species of the monogeneric or minimally polytypic family more or less corresponds to that between genera of the polygeneric family.[40]

Accordingly, Atran calls basic-level folk taxa *generic-specieme*.[41]

Generic-speciemes are psychologically prior to but immediately included in *life form* taxa. Life forms usually number no more than ten and are the most inclusive group of organisms 'recognized on the basis of numerous gross morphological characters.'[42] Brown's study of folk zoological life form terms in 112 languages revealed that five categories consistently occur based on whole images of gross morphology. Definitional criteria were found to take precedence over nomenclature as a means of recognizing folk zoological life-forms. Among the vertebrates these were fish, bird, snake and mammal. A residual category he called 'wug' delimited invertebrates and sometimes the smaller vertebrates(always includes insect and spiders; sometimes worms, occasionally snails, crabs, lizards, tortoises and small frogs).[43] Lexically, life forms are encoded

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Evidence from Aguaruna Jivaro Ornithology', *Journal of Ethnobiology* 1 (1981) 96 and 198, n. 2.

[40] Atran (1990) 28–9. Atran notes that Berlin had already recognized this phenomenon.

[41] 'Specieme' was the label given to the fundamental taxa of folk classifications by Ralph R.N. Bulmer, who maintains that they are recognized by numerous attributes of appearance and behavior. See Ralph R.N. Bulmer (1968) 'Worms that Croak and Other Mysteries of Karam Natural History', *Mankind* 6 (1968) 621–39; R.N. Bulmer (1970) 'Which Came First, the Chicken or the Egghead?', in *Echanges et Communications: Melanges afferts a Claude Levi Strauss a l'occasion de son 60eme anniversaire*, eds. Jean Pouillon and Pierre Maronda (The Hague: 1970) 1069–91; Ralph R.N. Bulmer in Ellen and Reason (1979) 57–79: 'Mystical and Mundane in Kalam Classification of Birds',; and with M.J. Tyler (1968) 'Karam Classification of Frogs', *Journal of the Polynesian Society* 81 (1968) 472–99.

[42] B. Berlin (1973) 'Folk Systematics in Relation to Biological Classification and Nomenclature', *Annual Review of Biology and Systematics* 4 (1973) 261.

[43] C.H. Brown (1979) 'Folk Zoological Life-Forms; Their Universality and Growth', *American Anthropologist* 81 (1979) 791–817.

with recourse to one or more attributes, the most common being size, behavior, and habitat. Given the importance of morphology (which in folk systems includes habitat and behavior) in the delimitation of non-residual life forms it is not surprising that, superficially, they correspond to scientific classes. However, 'For folk, life-form groupings represent locally distinct modes of life, ecological roles and morphological frames . . .'[44] For the scientist, they represent genealogical lines, as do the classifications in which they are imbedded. Atran notes that despite the role played by morphology in man's conceptualization of life forms, they primarily 'mark the boundaries living things have in relation to one another and in relation to ourselves.'[45]

Investigations in folk biology revealed the existence of taxa more inclusive than that of the generic-specieme, but not on the same level as those of life form. Berlin referred to this as 'intermediate' level.[46] Taxa at this level are rare and frequently unlabelled. Hunn cites the example of folk English 'duck,' a kind of bird (life form) that immediately includes several forms (generic-specieme) such as mallard, canvasback, scaup, etc., which may be partitioned even further.[47] Atran refers to these as 'family fragments,' and notes that they correspond only to *fragments* of a scientific family order, failing to completely delimit all local flora and fauna.[48] Suprageneric folk taxa seem to be especially recognized on the basis of the habitat they occupy, but the particulars of habitat more weakly define the groupings than the well-bounded perceptual aspects which underlie generic-speciemes. They are also less well defined by morphology than life forms and generic-speciemes. Because of this, the members comprising taxa at this level often are associated with more than one life form, in a sense operating independently of the phenomenal configurations which delimit life form taxa. Despite the often covert status of family fragments, Atran stresses their importance 'to an ordinary understanding of local nature, and more crucial still to historical attempts to transcend the limits of that understanding.'[49] That importance will, I hope,

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[44] Atran (1990) 37.
[45] Atran (1990) 37.
[46] Berlin (1972) (1973) (1974) (1978) passim.
[47] E. Hunn (1977) *Tzeltal Folk Zoology* (New York: 1977) 42.
[48] Atran (1990) 68.
[49] Atran (1990) 41. In addition to (sometimes covert) family fragments Atran discusses monogeneric life forms (p. 38; see also see Figure 1.) which

be made clear in the next section which shows how folk taxonomy can aid lexical identifications in ancient texts.

To complete the discussion of taxonomic categories found in Figure 1, I turn now to *folk specifics.* These taxa, when they occur, are immediately included in generic-speciemes and are much less numerous than the latter. They usually occur as contrast sets of two or three members, rarely exceeding ten in number. Those that do represent animals of great cultural significance. Folk specific names are usually binomial expressions consisting of the generic-specieme name plus a modifier referring to the color, sex, size, etc., of the animal.[50] Examples in American English would be ivy, poison ivy, cabbage, skunk cabbage.

Folk varietal taxa partition folk specifics even further; and are rare in folk taxonomies often signalling categories of cultural importance. Like folk specifics, varietals appear to reflect partitioning along a particularly salient phenomenal aspect. Examples in American English are baby lima bean and butter lima bean.[51]

A Case Study in Ancient Animal Identification Incorporating Folk Taxonomy

This section demonstrates the application of folk taxonomy to an interdisciplinary study of two related animal names, Ugaritic *anḫr* and Akkadian *nāḫiru,* neither of which is common in their respective lexicons. Ugaritic belongs to the group of northwest Semitic languages which include Hebrew (modern and ancient), various Canaanite dialects, Phoenician, Aramaic, and Syriac. Akkadian (east Semitic) is the generic name for the Babylonian and Assyrian languages of Mesopotamia which dominated spoken and written discourse between ca. 2800 and 600 B.C. *anḫr* probably dates from earlier texts and will be treated first.

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are conceptually similar in 'that they appear to have intuited aspects of both generic-speciemes and life-forms. Like generic-speciemes their facies are readily perceptible at a glance—as an immediately recognizable 'gestalt.' Like life-forms they occupy a distinctive role in the economy of nature. Because they are so distinctive, they may be easily marked *intensionally* by diagnostic characters chosen from dimensions spanning the other life-forms.'

[50] Berlin, Breedlove, and Raven (1966) (1968) (1973) (1974) passim. See the relevant works for linguistic exceptions.

[51] Berlin, Breedlove, and Raven (1966) (1968) (1973) (1974). A new book by Brent Berlin (1992) *Ethnobiological Classification. Principles of Categorization of Plants and Animals in Traditional Societies* (Princeton: 1992) was una-

Analysis of Ugaritic anḫr

The ancient city state of Ugarit located near the Mediterranean coast about seven miles north of modern Latakia, became an important commercial center in the third millennium B.C. Ugarit flourished in the fifteenth to thirteenth centuries because of important maritime connections. As a commercial entrepot the city housed a heterogeneous population—Egyptians, Hittites, Hurrians, and Mycenaeans among others, but was home to an indigenous group of northwest Semitic speaking people. A corpus of texts in Ugaritic written in a cuneiform script peculiar to the language are among the most important finds at the site. These date to the fourteenth century and include a variety of genres. Of interest here are the literary texts, particularly the mythological adventures of Baal the great storm god and principal deity of the pantheon. Based on stylistic conventions and the poetic language employed, the Baal Epic appears to be the oldest of the Ugaritic literary texts. The epic language of Ugaritic poetry preserves a grammatically and lexically archaic idiom that was no longer in every day use when the text was composed.[52]

Baal's combat with his mortal enemy Mot, god of death and the netherworld, is a central episode in the narrative. *anḫr* occurs in a difficult passage as part of a metaphoric description of Mot's appetite, which reveals his natural propensity for destruction. Mot issues an invitation to Baal through the latter's two envoys. They return to Baal with Mot's menacing message which begins with the passage in question. The simile is construed so as to explain Mot's yearnings for destruction as a natural, intrinsic part of his being, just as the behavior of

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vailable to me when I submitted this article to the editor in 1993. In this seminal work Berlin marshalls fresh evidence to strengthen his earlier arguments regarding folk biological classifications, categories and their rootedness in real-world structures. Although Berlin (pp. 76–78) takes exception to Atran's (1990) new label of 'generic-specieme', the system I adopted here, for his (Berlin's) generic taxon, the two are in fundamental agreement on the attributes and psychological salience of taxa of this rank. Likewise, Berlin's new discussion of higher order categories, especially covert taxa of intermediate rank (pp. 141–144) does not qualify my analysis (see following section) of two ancient Semitic animal terms which was predicated on earlier developed principles of such taxa.

[52] M. Held (1959) '*mḫṣ/mḫš* in Ugaritic and Other Semitic Languages, A Study in Comparative Lexicography', *Journal of the American Oriental Society*, 79 (1959) 175.

the other animals expresses a compelling inner drive innate to each creature respectively.

> A lion's appetite craves live prey
> An *anḫr's* appetite (desires) the sea[53]
>
> Behold (as) the buffaloes long for the water pool, the hinds long for the spring[54]

Two textual clues aid identification of the term, the first being *anḫr*'s position in the text. A key feature of Ugaritic poetry is the grammatical parallelism between two and sometimes three lines. The pattern of Ugaritic poetry rarely deviates from using more familiar language in line A while employing rare and poetic terms in line B.[55] This is clear in the passage under consideration where 'lion' in the A line was a common element of imagery while '*anḫr*' occurs only here.

The most important philological clue, however, is the semantic transparency of the word itself. *anḫr* derives from the triliteral root *nḫr* whose common Semitic meaning expresses snorting or snoring. Some initial *n* verbs in Semitic constitute a class whose morphology belie a semantic category: 'Verbs whose biradical basis without the element n, connotes a noise, e.g. Sem. *nbh/ḫ* 'to bark'..... *npḫ* 'to blow.'[56]

Although *anḫr* is included among other animals, it has seemingly little relation to them, since they are terrestrial. It also stands apart from other sea animals in that it respirates, thus confounding two out of the five universal life forms of fish and mammal. Logically, this has been the focus of all translation efforts. The identification of *anḫr* must be evaluated in light of the Akkadian term *nāḫiru,* since texts with the latter were discovered and translated many years prior to the Ugaritic corpus. Most Ugaritologists have equated the two terms as signifying the same animal, therefore *nāḫiru* has served as the basis for translating *anḫr.*

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[53] E.L. Greenstein (1973) 'Another Attestation of Initial *h*>' in West Semitic', *Journal of the Ancient Near Eastern Society of Columbia University* 5 (1973) 159–61.

[54] M. Held (1962) 'The YQTL-QTL(QTL-YQTL) Sequence of Verbs in Biblical Hebrew and Ugaritic', in *A.A. Neumann Festschrift* (Leiden: 1962) 290.

[55] Held (1962) 281, n. 2.

[56] S. Moscati (1969) *An Introduction to the Comparative Grammar of the Semitic Languages* (Wiesbaden: 1969) 169. Moscati attributes this observation to W. von Soden (1969) *Gundriss der Akkadischen Grammatik* (Rome: 1969) 137, 102b.

Analysis of Akkadian nāḫiru

Only a few attestations of *nāḫiru* occur in Akkadian texts, and those indicating some kind of sea animal are found only in royal inscriptions dating between ca. 1114 and 859 B.C. Several additional clues for identification appear in the Akkadian material, as will be seen. *nāḫiru* is first mentioned in the texts of Tiglath-Pileser I (1114–1076 B.C.) who consolidated the flagging Middle Assyrian empire by rejuvenating the army and conducting vigorous military campaigns. Two noteworthy precedents in literary style begin in the cuneiform inscriptions of this king. One is the appearance of 'annals' that give a (often year by year) purposeful account of the king's activities. The second is the insertion of the king's hunting expeditions into accounts of military campaigns, a theme imitated by later rulers. The royal annals reveal a keen Mesopotamian interest in exotic fauna. Kings repeatedly demanded strange and new animals be sent to them by conquered peoples.

1) One text describes the campaign of Tiglath-Pileser I to the Mediterranean and his conquests along the Phoenician coast (roughly contiguous with the modern Lebanese coast). He received tribute from the city-states of Byblos, Sidon and Arvad, an island city off the coast. He then set out in boats of the Arvadites for a hunting expedition at sea.

> In the midst of the sea I killed a *nāḫiru* which they call 'horse of the sea'[57]

2) There are numerous recensions of the following text which reports on military campaigns and includes a detailed description of building activities at the royal palace in the capital of Ashur. nāḫiru figures in the description of animal sculptures which were fashioned and erected at the palace gates.

> I made a basalt replica of a *nāḫiru*, which one calls horse of the sea, which I killed in the sea of Amurru with a harpoon(?), my handiwork, by the command of Ninurta and Palil, the great

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[57] All text citations are from A.K. Grayson (1976a) *Assyrian Royal Inscriptions, Part 2, From Tiglath-Pileser I to Ashur-Nasir-Apli II* (Wiesbaden: 1976) who provides a critical apparatus and translation for each text. This text is found on page 22 ff. of Grayson, text LXXXVIII 3. A more readily available translation is that of A. Leo Oppenheim (1969) in J.B. Pritichard (1969) *Ancient Near Eastern Texts Related to the Old Testament*, 3rd ed., ed. J.B. Pritchard (Princeton: 1969) 274–76. The collation of the cuneiform text is in E. Weidner (1957–58) 'Die Feldzuge und Bauten Tiglatpilesers I', *Archiv fur Orientforschung* 18 (1957–58) 342–60.

> gods, my lords; (and) a replica of a live yak(?) brought from the region of Lumash [. . .]. . . . . I erected (them) to the right and left of my [royal entrance].[58]

3 and 4) *nāḫiru* occurs in two inscriptions from the annals of the Assyrian king Ashur-bel-kala (1073–1056). They closely resemble those of his illustrious father Tiglatḫ-Pileser I, perhaps an effort to capture for himself the glory of his renowned forebear.

> The gods Ninurta and Palil who love his priesthood, entrusted to him open country hunting and so he rode in boats of the land of the Arvadites (and) killed a *nāḫiru* in the Great Sea.[59]

> I made (figures of) two *nāḫiru*'s, four yaks(?), four lions made of basalt, two protective spirits of marble and two yaks(?) of white limestone and had (them) erected at their gates.[60]

5) The last Assyrian king to mention the *nāḫiru* is Ashurbanipal II (883–859) in two almost identical passages which describe the tribute he received from two Mediterranean city-states, one of which was again Arvad. A key purpose of military forays in the ninth century was to obtain goods, particularly luxury items and building materials, for royal residences and temples which the kings listed proudly.[61] Here is one of the two passages.

> As tribute from the kings of the sea coast. . . . silver, gold, tin, bronze, bronze vessels, linen garments with multi-colored trim. . . . I received (as tribute), . . . teeth of a *nāḫiru* a marine creature.[62]

To summarize the additional Akkadian information: *nāḫiru* lived in the Mediterranean sea, was hunted by the king with a harpoon or spear from boats of the Arvadites, was called a

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[58] Grayson (1976a) 25–27, text LXXXVIII 4. Two texts of similar content regarding the building operations are found on p. 30, text LVXXXXVIII 5 (largely restored), and p. 34, text LVXXXVIIII 11. See also Oppenheim (1969) 275; text in Weidner (1957-58) 352:67–71. 'Amurru' is the general Akkadian term for (the) 'west.'

[59] Grayson (1976a) 46–48, text LXXXIX. Text from L.W. King and E.A. Wallis Budge eds. (1902) *Annals of the Kings of Assyria* Vol. 1 (London: 1902) 138.

[60] Grayson (1976a) 56; King and Budge (1902) 146–47;16–19.

[61] A.K. Grayson (1976b) 'Studies in Neo-Assyrian History, The Ninth Century B.C.', *Bibliotheca Orientalis* 33 (1976) 134–45.

[62] King and Budge (1902) 201 IV: 37-39 and 373 III:88.; Grayson (1976a) 149. Grayson's translation makes little sense here; see Wapnish (1985) 230, n.1 for discussion.

'horse of the sea,' and its teeth were prized as tribute. *nāḫiru* literally means 'snorter' in keeping with the meaning of the Semitic root. Though *nāḫiru* as an animal is infrequently attested, *nāḫiru* 'nostril' is more common.[63]

On the basis of its name, the Akkadian animal term was probably borrowed from northwest Semitic. An infinitive *naḫāru* 'to snore' is rarely attested in Akkadian suggesting it was a back-formation or denominative verb from the more common noun forms. The Mesopotamians, who had little knowledge of the high seas, would not have come into contact with such a creature were it not for the Arvadites, a sea-faring Phoenician people. It is not a coincidence that the first mention of it comes when Tiglath-Pileser went hunting in Arvadite ships. Would the Assyrians have had a name for it? Unlikely, for by this time (in fact long before) 'all tradition of marine fishes from Babylonia had been lost for along time.'[64] It seems likely that *nāḫiru* as the name of an animal entered Akkadian as a loan word from speakers of northwest Semitic, perhaps one of the Canaanite languages, not earlier than the reign of Tiglath-Pileser I.

nāḫiru has had a long history of translation since it was first encountered in cuneiform texts. Dolphin, suggested in the earliest Assyrian dictionary and based on 19th century translations, probably remains the most widely cited identification.[65] However, in 1907 Haupt wrote a well-reasoned argument in favor of the sperm whale, noting that a dolphin would not be much of a challenge for the Assyrian King.[66] In 1934 Landsberger maintained that 'sea horse' was an unsuitable epithet for the sperm whale, perhaps resulting from a confusion with the Mediterranean monk seal. The monk seal suggestion never caught on, probably because the Assyrian king would not have boasted of its killing as a heroic deed, as Landsberger himself noted.[67] The general tenor of Landsberger's remarks favored

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[63] See *The Assyrian Dictionary*, Vol 11, part 1. (Chicago: 1980) 137.

[64] Landsberger (1960) 80.

[65] W. Muss-Arnoldt (1905) *A Concise Dictionary of the Assyrian Language* Vol. II (Berlin: 1905) 665.

[66] P. Haupt (1906–07) 'Die Assyrisch Name des Potwals', *American Journal of Semitic Languages and Literatures* 23 (1906–07) 253–55. An English version appeared as 'Jonah's Whale,' *Proceedings of the American Philosophical Society* 46 (1907) 151–59.

[67] B. Landsberger (1934) *Die Fauna des Alten Mesopotamien nach der Tafel der Series HAR-RA = Habullu.* vol. 42 of *Der Abhandlungen der Philogisch-*

sperm whale, even though he emphasized that it was yet to be proven that the Assyrian king could hunt such an animal with a harpoon.

Weidner's discussion of *nāḫiru* questioned the identification of sperm whale. He doubted that the Assyrian king, already dependent on Phoenician navigational skill would have attempted to kill so huge an animal. He also points to the rare occurrence of the animal in the Mediterranean and questions if it would have received a popular name ('horse of the sea'). Finally, Weidner dismisses the idea that an odd sperm whale would have been coincidently in the vicinity of Arvad just as the king set sail. He rules out the monk seal and the dolphin since neither has teeth suitable for use as substitute ivory. On the recommendation of a zoologist he suggested killer whale, since, in his opinion, it fit the criteria better than any other available animal.[68]

In the inscription of Tiglath-Pileser I's expedition to the west, Oppenheim translated the term as narwhal.[69] But in his translation of Ashurnasirpal II's campaign to the Lebanon he renders 'ivory from walrus tusk.'[70] A third translation by Oppenheim yields killer whale.[71]

Yet another identification, hippopotamus, was proposed by Brentjes. I will return to several points he raises below, but for now will note that he leaves open the question as to whether Tiglath-Pileser I killed a whale or a hippo.[72]

Historischen Klasse der Sachsiscen Akademie der Wissenschaften 6 (1934). B. Meissner (1937) 'Studien zur assyrischen Lexikographie III', *Mitteilungen der Altorientalischen Gesellschaft* 11 (1937) 84–98, also translated sperm whale, with seal an alternative.

[68] Weidner (1957–58) 355–56; Haupt (1906–07) 254, claims that one authority meant a killer whale with his translation as grampus (whale).

[69] A.L. Oppenheim (1979) 275, n.3. The identification is odd because he cites Landsberger, who favored sperm whale, though not being completely sure. Narwhal is an alternative proposed for the Ugaritic term by A. Caquot, M. Szyncer and A. Herdner (1974) *Textes ougaritiques Tome I: Mythes et legendes* (Paris: 1974) 241–42.

[70] In this instance Oppenheim (1979) 276, n.17 cites Haupt, the original proponent of sperm whale. It is perplexing that Oppenheim twice cited authors favoring the sperm whale identification but then went on to offer a different animal with no further word of explanation.

[71] A.L. Oppenheim (1977) 'Man and Nature in Mesopotamian Civilization', *Dictionary of Scientific Biography* 15 (1877) 634–66. Oppenheim here cites Weidner who first proposed killer whale, so this time at least the citation is coordinated.

[72] B. Brentjes (1967) 'Maritime Saugetiere in den Kulturen des Alten Orient', *Zeitschrift fur Saugetierekunde* 32 (1967) 114–25. Hippo was already

Salonen, in his publication on the fish of ancient Mesopotamia, gives every indication of accepting the identification of sperm whale.[73] However, in a later study of hunting he suggests humpback whale, claiming that this identification is based on an explicit mention by the Babylonians of the animal's humped back, by use of Akkadian ***kutallu***. Moreover, he notes that the 'bones' of this animal are explicitly named by ***šinni***, which always means 'teeth' in Akkadian, but which he takes as a euphemism for bones.[74] Both of these arguments must be rejected. For the physiognomic feature 'humpback' Salonen cites ***kutal nāḫiri*** 'humpback of the n.,' and gives a leading Akkadian dictionary (by von Soden) as the reference,[75] which is totally misleading. The von Soden entry is for ***kutallu*** 'back of the head, back,' and while the passage is indeed listed it is for ***kutal nāḫiri*** 'back or side of the nostril.'[76] The passage comes from a corpus of texts concerned with the use of physiognomy for mantic purposes, and has nothing whatsoever to do with marine mammals. Salonen further claims that this idea for the humpback whale comes from the zoologist Hatt,[77] another misconstrued reference. Hatt merely notes the occurrence of the humpback whale (*Megaptera indica*) in the waters of the Persian Gulf and that 'it may be considered an Iraq record.'[78]

Regarding the inference of 'bones' from ***šinni***, 'teeth', of the text, it becomes clear why Salonen required this reading: humpbacks are baleen whales and have no teeth. Baleen, also called whalebone, is a horny, elastic material that hangs in plate-like sheets from the upper jaw of the Mysticeti, or baleen whales. It serves as a fine filter for the minute plankton on which they feed. Salonen is not alone in this interpretation, for Butz also considers that the 'teeth' are 'baleen' but, unlike Salonen, offers nothing to substantiate this equation (see below).

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proposed by Hommel in 1889 as noted by Haupt (1907) 147, n.3. Haupt regarded this identification as incredible.

[73] A. Salonen (1970) *Die Fischerei im alten Mesopotamien nach Sumerisch-Akkadischen Quellen*, vol. 3 in *Annales Academiae Scientiorum Fennicae*, Series B, (Helsinki: 1970) 170, 214, and 249.

[74] A. Salonen (1976) *Jagd und Jagdtier im Alten Mesopotamien*, vol. 196 in *Annales Academiae Scientiarum Fennicae*, Series B, (Helsinki: 1976) 218–19.

[75] W. von Soden (1965–77) *Akkadisches Handworterbuch*, Vol. 1, A-L, Fasc 1–13 (Wiesbaden: 1965–77) 517b. Salonen had already proposed this as a citation for the term in Salonen (1970) 249.

[76] Also cited by *The Chicago Assyrian Dictionary*, N/1, s.v. *nāḫiru* 'nostril.'

[77] Salonen (1976) 218.

[78] R.T. Hatt (1959) *The Mammals of Iraq. Miscellaneous Publications*, Museum of Zoology, University of Michigan No. 106 (Ann Arbor: 1959) 92.

On this basis he suggests two other baleen whales, similar to the humpback, the blue whale, and Bryde's whale. He notes that the humpback's tendency to cluster around shorelines makes it one of the easiest to hunt.[79] Butz further mentions the infrequent occurrence of whales on the Iraqi coast, and from this I can only surmise that he has paid little attention to the cuneiform evidence or is geographically confused. He takes no cognizance of the etymology of *nāḫiru* and its probable derivation from northwest Semitic; the unfamiliarity of Mesopotamians with seas and seafaring boats; the unlikely possibility that they would have a popular name for the animal; the fact that their knowledge of it centers on the Mediterranean and the people of its coast in whose boats they hunted it; and that it was apparently hunted not along a coast, but in the sea. Butz also claims that Weidner's identification of killer whale derives from the animal's prominent dorsal fin, or blade. Weidner, so he believes, was thinking of the narwhal with its spiral tusk, 'der aber nicht vorkommt.'[80] This is pure conjecture, and does not address the reasons Weidner advanced for killer whale, which are a cogent interpretation of the cuneiform evidence. Weidner nowhere mentions or alludes to narwhal and Butz's remarks must be rejected as irrelevant.

The Chicago Assyrian Dictionary offers three translations of *nāḫiru*: killer whale (?), dolphin and whale.[81] To review the list of proposed identifications in this tortuous translation history, they are: dolphin, sperm whale, killer whale, narwhal, walrus, humpback whale, hippopotamus, and monk seal. We turn now to a consideration of 'what was out there' that could best fit the textual clues.

Marine Mammals of the Mediterranean

It is clear we are dealing with some form of marine mammal that lived in the Mediterranean, as most philologists surmised. The Ugaritic and Akkadian texts pointedly mention the sea in connection with both animals, and together with the root meaning of the names we may concluded that these attributes were

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[79] K. Butz (1977) review of *Jagd und Jagdtiere* by Armas Salonen, in *Bibliotheca Orientalis* XXXIV (1977) 288–90, especially 287, n.52.

[80] Butz (1977).

[81] *Chicago Assyrian Dictionary* B, 329 s.v. *binūtu* for killer whale(?); *Chicago Assyrian Dictionary* D, 38 s.v. *daku* for dolphin; *Chicago Assyrian Dictionary* N/1, 137 s.v. *nāḫiru* for whale.

the focus of the animal category. The majority of animals proposed for *nāḫiru* correspond to modern taxa of the order *Cetacea*, 'whale' in folk English. A higher category of this magnitude includes animals of heterogeneous appearance, to which dolphin, sperm whale, killer whale, humpback and narwhal testify.

Many taxonomists recognize two suborders of *Cetacea*, the *Odontoceti*, or toothed whales, and the *Mysticeti* or baleen whales. The suborder of toothed whales is generally divided into six families. The three that concern us here are the *Monodontidae* which includes the narwhal, *Physeteridae*, the sperm whales, and *Delphinidae*, including the dolphin and killer whale.[82] I will begin with the least likely candidate, the narwhal.

The narwhal, *Monodon monoceros*, is known chiefly for its long spiral tusk which develops only in the male. Teeth of the female do not exceed 20 cm and remain imbedded in the mandible. In the male, the right tooth does not erupt, while the left develops into a rodlike tusk reaching to a size sometimes one-half total body length (maximum of 2.5–3 meters). Spiralization occurs over time resulting in a twisted tusk. *All* authorities agree that the narwhal's distribution is circumpolar but limited to the Arctic ocean and adjacent rivers and seas, no further south than the 65 degree latitude. On very rare occasions they will enter the North Atlantic, which only serves to reinforce its strictly Arctic distribution.[83] There was an additional attempt to locate the narwhal in the Mediterranean by Biblical commentators suggesting that it was the *tahash*, an animal whose skin was used as a covering for the tabernacle (Ex. 35:25; 36:13,etc.), and for sandals by the Israelites during their wanderings in Sinai (Ez. 16:10).[84] This conjecture contradicts all available information of the narwhal's distribution and should be rejected. The narwhal may likewise be ruled out as an identification of the *nāḫiru*.

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[82] A. DeBlase and R.E. Martin (1974) *A Manual of Mammalogy* (Dubuque, IA: 1974) 173–75.

[83] A.G.Tomilin (1967) *Mammals of the U.S.S.R. and Adjacent Countries*, vol. 9 in *Cetacea*, trans. by Omri Ronen (Jerusalem: 1967) 698–707. See also De Blase and Martin (1974) 175; E.J. Slijper and D. Heinemann (1977) 'Waltiere.-Die Zahnwale,' in H.C. Bernard Grzimek (1977) *Tierleben, Enzyklopaedie der Tierreiches*, Vol 11 (1969) 489; R.R. Reeves (1977) 'Hunt for the Narwhal', *Oceans* 10 (1977) 50–57.

[84] The late zoologist I. Aharoni claimed that the narwhal's habitat included the area between the 70th and the 80th degree of north latitude of the Red Sea: I. Aharoni (1944) 'Animals Hitherto Unknown to or Little Known from Palestine', *The Zoological Society of Egypt, Syria/Palestine*, Bulletin Supplement 6 (1944) 40–41.

The sperm whale belongs to the family Physeteridae which comprises two genera, *Physeter*, sperm whale, and *Kogia*, pygmy sperm whale. The genus *Physeter* is monotypic with only one species, *Physeter macrocephalus*, the largest of the toothed whales. Truly enormous in size, males attain a length of 18–21 m, females 12 m. The maxilla has eleven pairs of small teeth, frequently unerupted, particularly in the young. At least eighteen pairs of large, simple, peg-like, slightly recurved teeth are present in the mandible. The teeth of adult animals are without enamel and can attain a length ranging from 20–27 cm. Only one-quarter to one-third of the tooth is visible above the jaw line.[85]

Sperms prefer deep water, generally not less than 100 m deep. They are sighted close to land only where the coast drops off sharply into water,[86] and therefore are high sea creatures. Male sperms inhabit the world's oceans, generally limited by the 300 m isobath. Females and calves usually do not range beyond 45 degrees of latitude in either hemisphere. The distribution of sperms depends mainly on the availability of their major food source, cephalopod mollusks, and on suitable breeding grounds. Because Cephalopoda prefer tropical waters with a salinity above 30%, they are most abundant in the equatorial and temperate seas. The distribution of sperms is concentrated in the same area.[87] Regarding its appearance in the Mediterranean, both Haupt[88] and Weidner[89] cite Pliny (9,4) who believed that the sperm whale was infrequent in the Mediterranean and unable to go through the Straits of Gibraltar. However, Berzin writes:

> The narrow but deep Strait of Gibraltar is crossed by sperm whales mainly in spring. Together with the strong sea current they reach the Mediterranean Sea, with its rich fauna in cephalopod mollusks. Relatively numerous sperm whales arrive in this sea also in December–February (Bolognari 1957).[90]

Aharoni reported that sperms are found far to the east, near Alexandria and off Palestine,[91] which Bodenheimer corrobo-

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[85] Sperm whale teeth provided the raw material for scrimshaw, a popular carving medium of New England whalers. In addition, the mandible, a hard, almost ivory-like bone, was fashioned into articles such as a walking stick and small handles.

[86] D.J. Coffey (1977) *Dolphins, Whales and Porpoises: An Encyclopedia of Sea Mammals* (New York: 1977) 18.

[87] A.A. Berzin (1972) *The Sperm Whale* (Jerusalem: 1972).

[88] Haupt (1906–07) 261.

[89] Weidner (1957–58) 356.

[90] Berzin (1972) 169.

[91] Berzin (1972) 169.

rated by noting that sperm whales are occasionally sighted and vertebrae washed ashore.[92] In sum, we may conclude that sperm whales are not frequent, but occasional visitors to the eastern Mediterranean, unlike Weidner who concluded that their appearance was rare.

The family *Delphinidae*, the ocean dolphins, comprise seventeen genera with perhaps forty species, but the classification of genera and species changes regularly.[93] These have a worldwide distribution. The common dolphin, *Delphinus delphis L.*, is very frequent in the Mediterranean. This is the species so well known to ancient Mediterranean peoples, particularly the Greeks. The common dolphin has a slender streamlined body which does not exceed 2.5 m in length. The numerous teeth are small, a maximum of 10 mm high and 3 mm thick, very sharp and covered in enamel. In addition, to this dolphin, there is the blue-white dolphin, *Stenella caeruleo albus*, which occurs in the Mediterranean near the Algerian coast. It has numerous (160–200) small teeth about 3 mm. long and a body length not exceeding 2.4 m. *Grampus griseus*, or Risso's dolphin, is generally rare and mostly limited to the western Mediterranean. *Globidephalus*, the common pilot whale, has a widespread distribution including the Mediterranean. Maximum body length is up to 6 m. There are seven to thirteen pairs of teeth in either jaw that are small, pointed and conical, and do not exceed 12.5 mm in diameter.[94] Coffey classes the pilot whales and the killer whales in the family *Globicephalidae*, a family separate from the Delphinidae, since these members have fewer teeth, fewer vertebrae, more than three fused cervical vertebrae, and no beak, as do other members of the latter family.[95]

Orcinus orca, the killer whale, is usually described as a member of the family Delphinidae. It has a greater worldwide distribution than any other member of the family. They are also the largest members, reaching 10 m in length. The teeth are massive and strong, covered with enamel, with large roots firmly embedded in the mandible. From ten to thirteen pairs are found in each jaw, about 133 mm high in males (110 in females)

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[92] F.S. Bodenheimer (1935) *Animal Life in Palestine* (Jerusalem: 1935) 460.
[93] T.E.Lawlor (1976) *Handbook to the Orders and Families of Living Mammals* (Eureka CA: 1976) 205.
[94] Tomilin (1967) 466–515, 523, 574–92.
[95] Coffey (1977).

and 45 mm in diameter. They have no beak, so the head is like that of the porpoise. Although they occur worldwide, killers prefer cooler, and as a rule, shallow water. Tomilin reports that they are found in the European Mediterranean, but mentions no sightings in the east.[96] Unfortunately, the worldwide population of *Orcinus* is not known. If this is true, Weidner's source who suggested killer whale must not have realized that modern sightings are limited to the western Mediterranean.[97]

Pseudorca crassidens, or false killer whale, is slenderer and shorter than the killer, reaching up to 4 m in length. The teeth are much smaller with a mean length of up to 38 mm. Eight to eleven or twelve pairs are present in each jaw. It is widely distributed, and Tomilin notes a sighting off the Egyptian coast in 1956.[98]

Since I do not consider the humpback, a baleen whale, as a viable candidate for the *nāḫiru,* both for the reasons already presented and other that follow, this completes the possibilities of Cetacea.

The Mediterranean monk seal, *Monachus,* belongs to the order *Pinnipedia,* a group of mammals spending most of their time in water but with some attachment to land, mostly for birthing and nursing the young. Three modern families of Pinnipeds are recognized, *Phocidae* or the true seals, *Otariidae* or the eared seals, and *Odobenidae,* the walruses. The monk seals are phocids with three living species, one each in Hawaii, the Caribbean and the Mediterranean. The monk seal was well known to Mediterranean peoples, and there is evidence from ancient writers that seal hunting was quite an important activity in classical Greece. Adults are about 8–9 feet long. The upper incisors are large and pointed, larger than those of a big dog; canines are an average 28–30 mm in length.[99] The monk seal is found throughout the Mediterranean area, and apparently was quite abundant in the past.[100] In 1930, Aharoni noted that the seal was often offered for sale by fishermen from Ashkelon and Jaffa.[101] In 1934 Bertram reported that the range of its

[96] Tomilin (1967) 605–26.
[97] Weidner (1957–58) 356.
[98] Tomilin (1967) 599–603.
[99] J.E. King (1956) *The Monk Seals (Genus Monachus), Bulletin of the British Museum (Natural History), Zoology* 3 (1956) 204–06.
[100] A review of its past distribution is found in A. van Wijngaaden (1962) 'The Mediterranean Monk Seal', *Oryx* 6 (1962) 270–73.
[101] King (1956) 206.

existence was poorly known, and related several sightings of individuals near Haifa, and one off El Arish in 1941.[102] Bertram concluded that the last monk seal population was at the headland of Ras En Nagura, at the boundary of Palestine and the Lebanon. By 1966, Kumerlove, also noting the reduced monk seal population, reported sightings of a small number in the shore areas near Beirut, along the coves of Pigeon Grotto.[103] In 1973 I saw a small group clustered on the rocks off-shore of Pigeon Grotto as well. The Mediterranean monk seal is now endangered due to human intervention.

Odobensus rosmarus, the walrus, is the largest of the pinnipeds. Adult males are 3m or longer and may weigh up to 1400 kg. The tusks, which are the upper canines, extend beyond the lips and may reach a length of 100 cm weighing over 5.5 kg. All authorities agree that walruses are limited to northern seas.[104] In historic times walruses reached as far south as Nova Scotia, on the latitude of Milan Italy. During cold periods of the Pleistocene walruses are known from South Carolina and Bay of Biscay beaches,[105] but none have ever been connected to the Mediterranean.

The dugong, *Dugong dugong*, is a member of the order Sirenia, which like whales are fully aquatic mammals. Dugong are placid creatures, feeding on vegetation and preferring quiet estuarine waters and coastlines. Incisors in each jaw can be tusk-like in the males but small and often unerupted in females.[106] The family *Dugongidae* ranges through the coastal waters of the Indo-Pacific region. In modern times they occur in the Red Sea extending all the way north to the Gulf of Aqabah.[107] Dugong were a mainstay of the food supply at the site of Umm

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[102] G.C.L. Bertram (1943) 'Notes on the Present Status of the Monk Seal in Palestine', *Journal of the Society for the Preservation of the Fauna of the Empire* 47 (n.s.) (1943) 20–21.

[103] H. Kumerlove (1966) 'Zum Vorkommen der Monchrobbe, *Monachus m. monachus* (Hermann, 1779), im libanesischen Kustengebiet', *Saugetierkundliche Mitteilungen* 11 (1966) 114–18.

[104] V.B. Scheffer (1958) *Seals, Sea Lions and Walruses, A Review of the Pinnipeda* (Stanford: 1958) 18.

[105] F. Bruemmer (1977) 'The Gregarious but Contentious Walrus', *Natural History* (Nov. 1977) 52–60.

[106] DeBlase and Martin (1974) 227–28; Lawlor (1976) 208.

[107] G.C.L. Bertram (1943) 'Note on the Sea Cow in the Gulf of Aqabah', *Journal of the Society for the Preservation of Fauna in the Empire* 47 (n.s.) (1943) 21–23.

an-Nar on the Persian Gulf in the third millennium. They are rare in the area today.[108]

Hippopotamus amphibius belongs to the order of Artiodactyla, the even toed ungulates. Hippos are river dwelling mammals of African origin. They feed mainly on aquatic plants so much of their time is spent in the water. They can remain submerged for up to five minutes at which time they must surface to breathe. Hippos are huge animals, standing about 160 cm at the shoulder and weighing up to five tons. The skull is massive with a wide mouth containing large lower tusks, the canines, which grow throughout life, and large upper and lower incisors.[109] Hippos are known from prehistoric and historic period sites in Israel, where their teeth were used for manufacturing small artifacts.[110] The remains of hippo tusks have also been reported from the excavations at Ras Shamra (Ugarit) from the thirteenth to fourteenth centuries B.C.[111]

Before leaving this exposition of possible marine mammals, it is appropriate to consider the popular name of the *nāḫiru*, 'horse of the sea.' Can this epithet be attributed to the horse's snorting or to its tendency to rear up on its hind legs when frightened or excited? During the middle Babylonian period (ca. 1500 B.C.) names from the root *nḫr* were applied to horses, though rarely. All sea mammals will snort to clear their nostrils of water before taking in fresh air, so in this sense no one candidate is more suitable than another. However, some will snort more conspicuously, for example the dolphin versus the sperm whale, but this again leaves mere conjecture.

Breaching by the Cetacea may be likened to a horses rearing back, but in action only. It has nothing to do with high spirits, but is rather a requirement of life. Dolphin jumps are quite different from those of the larger whales. Dolphins travel at high speeds and must breathe often, so the jump must clear the water for it to be effective. The jump has a low trajectory which allows the animal to lessen the amount of water resistance that results but is unavoidable with partial emergence. Excess resistance would slow their speed below the critical level

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[108] G. Bibby (1969) *Looking for Dilmun* (New York: 1969) 281–82.

[109] DeBlase and Martin (1974) 238.

[110] I have identified hippo bones and teeth from Tell Ta'anach, Tell Qasile and Ashkelon. These date from the ca. 1700 to 300 B.C.

[111] G. Naas (1953) 'On the Occurence of Hippopotamus in the Iron Age of the Coastal Area of Israel (Tell Qasileh)', *Bulletin of the American School of Oriental Research* 132 (1953) 30–34.

needed to ride the waves.[112] When the sperm whale breaches, its body is inclined backward to the water's surface at about a 45 degree angle. As it falls, it turns sidewards onto its belly. Sperms usually breach two or three times in rapid succession probably after deep submersion. Most authors note that it does not emerge fully from the water.[113]

It would be pointless to speculate if one or the other of these breaches were more or less like the horse's action. Likewise, none of the observations permit us to certify if snorting or breaching motivated the popular epithet. They do, however, give the edge to the Cetacea over the monk seal and the hippo.

THE ARCHAEOLOGICAL EVIDENCE

Save for the dugong bones recovered from the Persian Gulf site of Umm an-Nar mentioned above, no bones of marine mammals have been reported from archaeological sites in the Near East. I know of only one exception, an as yet unpublished lumbar vertebrae from a (rather small) dolphin recovered from Early Bronze Age contexts at the inland site of Ai, located on the eastern slopes of the central hill country in Israel.[114] Hippo bones and teeth have been recovered as noted above, but their numbers are small. Ivory is the only faunal material that will provide additional evidence for the presence of these animals.

The Near Eastern ivories studied are almost all from the African or Asiatic elephant. Some worked hippo tusks are also known, but these are rare. It is possible, but not easy, to distinguish elephant and hippo ivory, but this is almost never attempted with Near Eastern artifacts. Ostensibly, ivory from the African or Asiatic elephant is also distinguishable, but in practice very difficult to do especially on weathered artifacts. This too has been little attempted. Needless to say, there have been no efforts to distinguish artifacts that might have been made from sperm (or the less likely killer) whale teeth.[115]

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[112] L.H. Matthews (1980) *The Natural History of the Whale* (New York: 1980) 108.

[113] Berzin (1972) 262.

[114] The bone was identified by Brian Hesse and myself. We sent it to Elizabeth J. Reitz, a specialist on marine fauna at the Zooarchaeology Laboratory, University of Geogria, for confirmation. The bone could not be identified to species.

[115] The subject of ivory in the ancient near east is a study in itself. For a preliminary discussion see Wapnish (1985) 251–59.

Ivory was always a highly prized, luxury commodity in the ancient Near East, increasing in popularity until the Roman period when the enormous demand seriously affected the integrity of the source herds. In this light, there is little reason to believe that baleen plates would have been considered on a par with true ivory. Baleen bears no resemblance to teeth, and the possibility that the Akkadian word for teeth, which was how they named ivory (lit. 'elephant teeth'), also denoted baleen stretches the imagination. Additionally, baleen cannot be used as ivory is used,[116] and the connection between ivory and its uses was a very strong one. The fact that both substances originate in the mouth is insufficient to override their very different physical properties and use. This inexorably removes the humpback whale from consideration, together with the zoogeographic evidence presented above.

ARTISTIC EVIDENCE

The inscriptions of Tiglath-Pileser I and Ashur-bel-kala tell of fashioning animal figurines and erecting them at their palace entrance at Ashur. Excavators found numerous fragments of animal figurines near the palace entrance and fragments of basalt slabs with inscriptions of Tiglath-Pileser I.[117] A basalt figurine of an unknown animal with an inscription of Ashur-bel-kala was also recovered.[118] None of the figurines from the Ashur excavations can be identified as any of the candidates for the *nāḫiru*. Brentjes (citing Harden) says it is certain no figure of the *nāḫiru* was ever made.[119] No other large whales are known to have been depicted.

Dolphins were popular animals in the art of the Aegean, but only one or two instances are known from the Near East.[120]

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[116] No baleen has been recovered in the Near East from an archaeological context, although the substance would be so rare and unfamiliar that the chances of its being recognized are slim. Baleen was recovered at a Canadian settlement where Arctic whalers covered raised flagstone platforms by a layer of the flexible, plastic-like baleen; see R. McGhee (1981) 'A Tale of Two Cultures: A Prehistoric village in the Canadian Arctic', *Archaeology* 34 (1981) 44–51.

[117] Grayson (1976a) 87; see Weidner (1957–58) 356–58, for a description and photograph of the fragments.

[118] See Grayson (1976a) 87, n.114 and p. 59.

[119] Brentjes (1967) 119.

[120] One example from Tepe Gawra in the early third millennium is doubtful; see E.A.Speiser (1935) *Excavations at Tepe Gawra*, Vol. 1 (Philadelphia: 1935) 137, Plate 84, No. 18. A fragment of a boundary stone is also said to

Jumping dolphins are depicted on a large crater discovered in the southern palace at Ugarit. It dates to the end of the thirteenth/beginning of the twelfth century B.C., and on the basis of technique, typology, theme and style, was probably of Mycenaean origin.[121] In sum, no extant artistic representations offer further evidence to identify the *nāḫiru*.

TECHNOLOGY OF MARINE HUNTING

Two Akkadian words specify weapons suitable for hunting the large animals under discussion. The *azmaru*, 'javelin,' was used to kill lions during the hunt.[122] *pasḫu*, the instrument used to kill the *nāḫiru* was also used to kill lions; only a few citations are known.[123]

It was not until the tenth/ninth century B.C. that the Assyrian king set out in ships from the Phoenician port of Arvad for a marine hunt.[124] A relief from Sennacherib's palace at his capital of Nineveh (701–681 B.C.) shows Phoenician warships and transports fitted with two tiers of oarsmen,[125] a design of Phoenician invention. Barnett distinguishes three or four types of Phoenician ships.[126] Since all were oared, these ships could not have given high speed chase to a cetacean; a certain amount of strategy would be involved to hunt a whale from one of these ships.

At least one author maintains that no technical means existed to hunt any of the larger sea mammals. He states that reports of aboriginal hunting in fifteenth century B.C. Alaska are in error; the evidence shows that they were wont to chase stranded animals ashore where they killed them.[127] This position fails to take into account the many reports of aboriginal whaling

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depict a dolphin, but it is too damaged for a positive identification, see E.D. Van Buren (1939) 'The Fauna of Ancient Mesopotamia as Represented in Art', *Analecta Orientalia* 18 (1939) 82.

121 AJ. Nougayrol, E. Laroche, C. Virroleaud, and C.F.A. Schaeffer, eds. (1968) *Ugaritica V* (Paris: 1968), Plates 3–7.

122 Salonen (1976) 28–29.

123 Von Soden (1965–77) 884; Salonen (1976) 51, s.v. *pariangu*.

124 The inscriptions of Tuthmosis III and later the Amarna letters reveal that Arvad was already a considerable naval power in the fifteenth century B.C.; see L. Casson (1971) *Ships and Seamanship in the Ancient World* (Princeton: 1971) 36.

125 Casson (1971) fig. 78.

126 R.D. Barnett (1958) 'Early Shipping in the Near East', *Antiquity* 21 (1958) 220–30.

127 E. Neufeld (1973) 'Fabrication of Objects from Fish and Sea Animals

which show, for example, how Eskimos of the Bering Strait hunted whales for at least the past two thousand years. The following is taken from Heizer's observations.

'All aboriginal whaling belongs, of course, to the category called 'shore-whaling' since ocean-going vessels for whaling are not used or known by any primitive people.'[128] Three different techniques for killing large whales are known. The Norwegians maneuvered whales into bays using nets, then shot them with a dart smeared with pathogenic bacilli. The whale sickened, died, and was dragged ashore for processing. In a second technique, a large oared or paddled boat with a crew of six to eight men, one the harpooner, approaches the whale as it surfaces. The animal is harpooned with a heavy instrument to which a long line is attached, appended with wooden drags or inflated skin buoys. The wounded animal dives, but is restricted by the drags. Upon resurfacing, it is harpooned again, and if sufficiently worn out, it is approached and a long sharp lance thrust into its body in hopes of hitting a vital organ. The dead animal is again towed ashore and processed. European and American whalers of the seventeenth to nineteenth centuries employed this method, as well as aboriginal hunters of the Arctic seas. The success of this technology by recent European and low-technology peoples bespeaks a parallel adaptation to the same basic conditions of a simple technology involving the sea and their marine habitat. A third technique involved harpooning the whale in the ocean with a lance dipped in aconite, a deadly poison. The dead whale then, hopefully, drifted to the shore in the whaler's territory; if not, another tribe was the beneficiary.

None of these techniques involves chasing the whale ashore. 'Shore-whaling' does not mean this was done close in, only not miles out to sea. In technique two, the party has to be far out enough for the whale to dive, and large whales require considerable depth. This technique would certainly have been within the potential technology of Phoenician seamen, who could have dispatched a smaller whaling party from the mother ship once the animal was sighted. We even know from Assyrian reliefs that they used inflated animal skins as buoys or floats,

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in Ancient Israel', *Journal of the Ancient Near Eastern Society of Columbia University* 5 (1973) 309–24.

[128] R.F. Heizer (1968) 'A Bibliography of Aboriginal Whaling', *The Journal of the Society for the Bibliography of Natural History* 4 (1968) 344–62.

that could have served as the drags. We don't know if this was a usual kind of hunt. Since no other evidence for it exists except that which is under discussion, it probably was a rare occurrence. Tiglath-Pileser's I harpooning of a *nāḫiru* may not have been a singular event, but it was not a common one either. However, claims that the technology for large whale hunting did not exist should be rejected.

THE IDENTIFICATION OF *NĀḪIRU*, ANḪR AND FOLK TAXONOMY

The foregoing discussion of the extra-philological evidence makes it clear that no single fact emerges to irrevocably identify *nāḫiru*. The picture must be fitted together from suggestive and logically possible pieces of evidence.

Walrus and narwhal are ruled out because they habit only northern seas. Dugong is inadmissable because it does not appear along the Mediterranean coast and prefers the becalmed waters of an estuarine environment. Hippopotamus, though known in the historic period of the Levant, would not stray far enough out from the Mediterranean coast for the Assyrian king to describe the hunt as being 'in the midst of the sea'. The humpback whale is ruled out since its back was not singled out for special mention as proposed by Salonen, and the notion of equating large teeth and baleen via the Akkadian word for 'teeth' is far-fetched. The Mediterranean monk seal and the dolphin would be excellent choices were not their teeth no more impressive than those of a terrestrial mammal of comparable size. The only candidates left are the large toothed whales.

Here we face two problems that many translators comment on but which can only be addressed subjectively. The first is virtually obviated now by the elimination of many of the possible candidates. I mention it because it has been used by other philologists, namely, the heroics of the Assyrian king. The dolphin, in particular, was deemed unsuitable for not presenting enough of a challenge. Assyrian kings were undaunted hunters of big game, but we should remember that sometimes the royal sport was played out in a not too large enclosure encircled by the king's armed retainers. Not much chance for the animal, and not an act of great bravery to modern eyes. Too, modern sportsmen consider catching big game fish a sizeable achievement, and these are certainly smaller than many dol-

phins. Finally, one wonders how much the king had to do with the actual hunt given Assyrian dependence on Phoenician expertise. It is more likely that he just moved in for the kill, leaving the more strategic and dangerous preliminaries to the Arvadites.

The second subjective assessment involves the descriptive phrase *ina qabal tâmti* 'out at sea.' Does it really mean out on the high seas or just at sea, not necessarily miles from shore? We have no way of knowing. *ina qabal* is too much of a stock phrase to be more precise and there is no way to judge what the Assyrian king's perception would have been. In terms of the technology of hunting, however, it should not make a significant difference. Even in shore whaling there must be enough area and depth to maneuver and avoid a wounded, plunging animal. We do not know if the hunt was carried out in smaller boats, but a large, oared, ship would have been a very difficult vessel for such a hunt.

Of the larger whales, the sperm whale is the most likely candidate, if one agrees to the technical possibility of hunting such a formidable animal. The killer whale ostensibly represents the best choice—large, fierce, massive teeth—but despite its world wide distribution in all oceans, there is no modern evidence for its occurrence in the eastern Mediterranean. The false killer whale, though known from this sea, is disqualified because of its less than impressive teeth. Since the sperm whale is infrequent in the eastern Mediterranean and the killer whale unknown, the extra-philological evidence will best contribute to a more positive identification of the *nāḫiru* with additional sightings of these marine giants.

Are *nāḫiru* and *anḫr* to be equated with the same animal? Here I disagree with most other commentators in that I believe the answer is no. Against the background of 'what is out there,' the Akkadian context would appear to indicate a folk taxon at the generic-specieme level. Sperm whale fulfills this criterion. But the sperm whale does not 'fit' the Ugaritic passage. It is altogether too infrequent for the framework of an explanatory metaphor, and no outside evidence for its occurrence is forthcoming. It therefore should be rejected as a gloss.

Given the animal possibilities, the dolphin or the monk seal are the two most suitable creatures for *anḫr*. As a mammal spending most of its time in the sea, the monk seal would make a snorting sound and was known in antiquity and from modern distributions to habit the Levantine coast. The common

dolphin was well known in Greece, and though known to Levantine peoples, appears to have been less culturally salient than in the western Mediterranean. The one depiction of it from Ugarit was an import from the Greek mainland. The frequency of these creatures conforms more to the situation of the Ugaritic passage—an uncommon, poetic 'B' word, but one denoting an animal not too rare or unfamiliar to obscure the total metaphor.

By examining *nāḫiru* within the Akkadian domain of wild animals we see that it is not found anywhere in the lexical lists.[129] In this case, its absence may not be entirely attributed to the fact that it was unknown and/or exotic to the Assyrians, a situation which may explain the deletion of several other animals from the list. More important was its status as a generic-specieme exhibiting characteristics of two life-form classes. The two attributes criterial for its definition crossed boundaries of major classes, i.e., its piscine form and watery habitat did not accord with its 'snorting.' It is not surprising that *nāḫiru* was not included in any of the five major life-form categories by which the list of wild animals was organized.

Thus, *nāḫiru* was cognized at Berlin's intermediate level, what Atran calls a family fragment, through a process of disassociation.

> Disassociation at the life-form level occurs most readily for generic-speciemes whose phenomenal affiliations with their respective life-forms are only marginal..... Most often, folk views on the extension of these life-forms differ from scientifically construed extension of the corresponding classes in regard to what, from the folk viewpoint, are rather marginal cases (and for natural history traditionally the most problematic): bat, ostrich, whale, etc. Indeed, as often as not these marginal groupings assume a separate life-form of their own; hence they are treated as monogeneric life-forms...... Porpoises do intrigue folk. They are evocative because, along with whales, they are phenomenally aberrant at the life-form level...[130]

nāḫiru and *aḫr* may not be cognates in that they do not denote the same animal. But folk taxonomic evidence strongly suggests the existence of a covert category at intermediate level either in Proto-Semitic or in each language respectively. The term may best be glossed 'marine mammal,' so singular is the

[129] Landsberger (1934) 141–42.
[130] Atran (1990) 68–69.

linkage between a fish-shaped aquatic animal and breathing in both east and west Semitic. When *nāḫiru* was borrowed from the Arvadites it was probably as a *Kultur*-word, one which denoted the same animal. However, the earlier west Semitic term did not label the same animal, but one with the same phenomenal aberrations. A lexical change had occurred within west Semitic. The process of lexical change within one language or between closely related languages is frequently predicated on an immediate transitive relationship between the two conceptualizations involving 'part of' or 'kind of' notions. In folk biological taxonomies, categories immediately linked by the transitive relationship 'kind of' are sometimes labelled by the same term. Within west Semitic or Canaanite (if one so classes Ugaritic) *anḫr* may have existed as a more inclusive mid-level category in addition to its status as a generic-specieme. This would explain how its meaning could shift within a closely bounded framework over several hundred years.

The Akkadians were not borrowing a word devoid of meaning, since *nḫr* was a root familiar to them. They borrowed the use of a familiar root to name an unfamiliar animal. In a folk taxonomic sense, a term behaving like *nāḫiru* which is not immediately included in one of the major life form classes, still gives us an important guide for identification. Contrast with other taxa could only occur at a higher node in the taxonomy, giving a depth to the classification that we would expect in a complex society. Here again we may speculate on the existence of a covert mid-level category. Now covert categories are hardly candidates for lexical borrowing, but because the same root with a similar semantic range existed in both languages, the apparatus for such a conceptual devolution was in place in Akkadian. This may or may not have occurred, or in borrowing a specific term the Akkadians may also have acquired the sense inherent in a mid-level category. What is clear, is that this process was more than a simple case of lexical borrowing.

Finally, folk taxonomy provides for a better identification of *anḫr* with recourse to *nāḫiru*. The finer lexical and extra-philological evidence that exist for the Akkadian term makes for a clearer identification and understanding of the relationship involved. Since folk taxonomic studies have documented the existence of mid-level categories and have shown that 'the biological plausibility of such taxa appears, in the main, to be as high as that for folk genera (in terms of their.... non-

violation of family boundaries),'[131] of the two animals proposed for *anḫr*, the monk seal or the dolphin, the latter would clearly be the proper choice in light of the identification of *nāḫiru* as one of the toothed whales. The psychological boundaries of mid-level taxa should reflect biological realities rather than the broader definition that was possible on the basis of the philological, archaeological, zoological and artistic evidence. 'Marine mammal' can now be redefined as '(toothed) whale'; the theoretical strength behind folk taxonomy supports the likelihood of an intermediate covert category in (perhaps) Proto-Semitic, Ugaritic and Akkadian, to be equated with the systematists Odontocetes, the sub-order of toothed whales.[132]

CONCLUSION

This paper began by outlining the need for archaeozoology to develop appropriate categories for analysis that are not informed solely by scientific referents. While the zooarchaeologist will always have to manipulate Linnaean categories to answer fundamental questions of subsistence and production, that is not the only goal in studying a zoocultural system. It is also true that understanding the folk category conceptualization of an animal can provide positive feedback for investigating materialist questions.

In this pursuit of appropriate analytic units, folk classification was cast as the mediator among zoological categories, ancient terms and animal bones. While the form of folk classification followed here, the folk taxonomy of the formalist school, may superficially resemble Linnaean categories because they both build on phenomenal attributes, the motivation between folk and scientific arrangements and the way in which different levels are intuited in each system, are substantively different. This allows us to appreciate how an animal can be different in each system, even when the same features contribute to delineating the respective category in each system.

The interaction between folk taxonomy and ancient terms

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[131] B. Berlin, letter of 22 March 1982.

[132] In the classification system I followed the Odontocetes were classed as a sub-order. Systematic categorizations at higher levels can sometimes be arbitrary and may vary by zoologists. This does not represent a problem, however, because the nature of the biological correspondence remains intact.

was seen when the operationalized, formal model describing folk taxonomic levels provided a predictability that allowed for a better identification of two related animal terms in ancient texts. An important part of the process was in defining the level of inclusiveness for the modern gloss. Now it is time to bring bones into the equation.

How to evaluate the dolphin bone found at Ai? Although we were not able to identify which Mediterranean dolphin the bone came from, the evidence from the Ugaritic and Canaanite texts makes this zoological task less important in terms of understanding the significance of that category in ancient Canaan. This does not mean going back to separate disciplines with the attitude that all information needed for identification can be found in the texts. If nothing else, the translation history of *anḫr* and *nāḫiru* should put the lie to that. It is clear, that without the interweaving of philology, folk taxonomy, zooarchaeology, and modern zoology we should never have come to an appropriate understanding of 'how' those ancient terms and bones 'mean.'

I should like to pre-empt criticism that I am advocating a cumbersome and complicated method by pointing out that my example dealt with two quite obscure terms of animals that were not highly salient in either culture. More common terms and animals, i.e., animals of economic importance, will not present the same difficulties, or at least the more focal members of their respective categories will not.

Zooarchaeologists working with historic period material have an advantage that our colleagues in prehistory do not: a much enhanced potential to access behavioral motivation. It behooves us to develop our own paradigms so that we may realize this potential in our study of the past.

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